THURSDAY, JUNE 20, 1895.

THE ATOMIC THEORY AND ITS AUTHOR.

John Dalton and the Rise of Modern Chemistry. By Sir Henry E. Roscoe, D.C.L., LL.D., F.R.S. Century Science Series. Pp. 212. (London: Cassell and Company, Ltd., 1895.)

WE have read through this little book from beginning to end with a great deal of pleasure. It tells the story of a life which has already been told more than once, but it tells it in a pleasant style, while at the same time it is fairly complete and, what is equally important in these days, not too long.

John Dalton was born at Eaglesfield, near Cockermouth in Cumberland, in 1766, about September 6; but as no register containing a record of his birth has been found, the exact date is not known. John is supposed to have been the second son of his parents, Joseph and Deborah Dalton, but, for the same reason, this statement cannot now be verified. According to his own account he attended the village schools in the neighbourhood, and was fortunate in attracting the notice of Mr. Elihu Robinson, a Quaker like his parents; but while Joseph Dalton was but a humble hand-loom weaver, Robinson was a man of independent means and considerable scientific ability. Under the influence of Mr. Robinson, John made such progress, especially in mathematics, that at the age of twelve he set up school teaching on his own account. When he was about fifteen he left his native place, in order to join his elder brother Jonathan in the conduct of a school at Kendal. Four years later, in 1785, George Bewley, the proprietor of the school, retired from the management, and John became his brother's partner. A quaint card, reproduced photographically in the book, announced to their friends and the public that youth would be "carefully instructed in English, Latin, Greek, and French, also writing, arithmetic, merchants' accompts, and the mathematics."

All this time John was diligently occupied in self-improvement. His active mind, however, could not be contented with mere acquisition of knowledge, and we find that his first attempts at scientific investigations were made here. Meteorological observations occupied him in the first instance, and the requisite barometers and thermometers were made with his own hands. This was the beginning of the long series of daily observations which were continued without a break until the evening before his death in 1844.

In 1793 Dalton left Kendal for Manchester, having undertaken for the modest stipend of £80 a year to teach mathematics, mechanics, geometry, book-keeping, natural philosophy, and chemistry, and we are told that in 1794 he had twenty-four students in these subjects. In this position of college tutor Dalton remained six years, and then resigned his post in order to obtain time for his researches, supporting himself by private tuition. When he left the college, he lived first in a house in Faulkner Street, then with John Cockbain, a member of the Society of Friends; but, after a time, joined the family of the Rev. William Johns, with whom he remained nearly thirty years. It was here that his most important original work in physics and chemistry was accomplished, here he brought out his system of chemical philosophy, and here he attained to that celebrity which brought him honours from abroad, as well as the friendship of the most distinguished of his own countrymen.

To the pages of the book we must refer our readers for many of the details of Dalton's subsequent career: how he delivered courses of lectures in Edinburgh and Glasgow (1807), and twice at the Royal Institution in Albemarle Street (1803–4 and 1809–10); how he was made a corresponding member of the French Academy of Sciences (1816), and a Fellow of the Royal Society (1822); how he visited Paris (1822), and subsequently, after the death of Davy, was elected a Foreign Associate of the Academy (1830); how he received honorary degrees from many universities, among the rest, from Oxford (1832); and, finally, was assigned a pension out of the funds of the Civil List by King William the Fourth.

Dalton died on July 27, 1844. Since 1837, when he had a paralytic stroke, his vigour had very seriously declined; and of this decline it is obvious that he was conscious. Old people are usually parsimonious, especially if in their younger days they have been obliged to practise economy. Dalton was no exception to this, and an amusing account, which will not bear condensation, is given of a transaction of his with Dr. Lyon Playfair, in January 1844, only a few months before his death.

Dalton seems to have been a great smoker. In a letter quoted on p. 166, he says (January 10, 1804):

"I was introduced to Mr. Davy, who has rooms adjoining mine in the Royal Institution. He is a very agreeable and intelligent young man, and we have interesting conversations in an evening. The principal failing in his character is that he does not smoke."

Wrapt as he was from early youth in his scientific and philosophical pursuits, it is perhaps not surprising that he should have declared that his head was "too full of triangles, chemical processes, and electrical experiments, &c., to think much of marriage." Nevertheless, it appears that the Quaker philosopher had at least one or two affairs of the heart, and even when past the age of giddy youth he seems to have been accessible to the charm of female beauty; for in a letter in which he describes "the belles of New Bond Street," he admits that he is "more taken up with their faces than their dresses," and ends with the remark, "I do not know how it happens, but I fancy pretty women look well anyhow."

Every one has heard of Dalton's peculiarities of vision. It seems remarkable that he should have grown to manhood without becoming aware of his defect, but it appears that it was not till about the age of six-and-twenty that he found out that his notions of green and red were different from those of other people. This evidently caused him at first a good deal of perplexity, and brought down a certain amount of "chaff," for he writes to his old friend Elihu Robinson, that "the young women tell me they will never suffer me to go into the gallery of the meeting-house with a *green* coat; and I tell them I have no objection to their going in with me in a *crimson* (that is, dark drab) gown." Dalton had a notion that his defect of vision was due to the existence of a coloured medium in one of the humours of the eye. It is almost needless to say that this was a mistake, and that the fact has now long been recognised that many persons are unable to distinguish red and green, though the true physiological explanation is still unknown.

We must now turn to a brief consideration of the chief subject of Dalton's scientific investigations. In connection with the history of the evolution of the atomic theory, Sir Henry Roscoe has been so fortunate as to make an interesting discovery. Among the "Dalton Papers" in the possession of the Manchester Literary and Philosophical Society, he has found the manuscript notes prepared by Dalton for the course of lectures delivered at the Royal Institution in the winter of 1809–10. In these notes he gives an account of the train of thought which led him to adopt the atomic hypothesis for the explanation of chemical phenomena. Contrary to the commonly received account, which appears to have originated with a statement by Dr. Thomas Thomson in his "History of Chemistry," the atomic theory did not first occur to him during his investigation of olefiant gas and carburetted hydrogen gas From the newly-discovered manuscript it appears that Dalton's atomistic ideas arose in the course of his study of the atmosphere, and in speculating as to how a mixture of two or more elastic fluids could constitute a homogeneous mass A reader of his "Chemical Philosophy" would perceive how thoroughly he was imbued with the Newtonian doctrine of particles, and in Henry's "Life" this is clearly pointed out.

By whatever process Dalton arrived at the adoption of the atomic hypothesis, it is certain that his great merit consisted in the application of a commonly accepted (see "Chemical Philosophy," part i. p. 141), but vaguely conceived, notion to the explanation of chemical phenomena. It was "for the development of the chemical theory of definite proportions, usually called the "Atomic Theory," more especially, that he received the first awarded Royal Medal in 1826. This is the point upon which emphasis was particularly placed by the president, Sir Humphry Davy, in presenting the medal.

In the course of reading this little book we have met with only one passage which seems to require correction. The statement (p. 153) that Dalton's "great achievement was that he was the first to introduce the idea of quantity into chemistry" is not only erroneous but is inconsistent with the writer's own text, which on p. 161 contains a reference to the names of Wenzel and Richter.

We shall look forward with pleasure to the other volumes of the series. W. A. T.

HYDRAULIC AND OTHER POWERS.

Hydraulic Motors, Turbines, and Pressure Engines. By G. R. Bodmer, A.M.Inst.C.E. Pp. 540. (London: Whittaker and Co., and George Bell and Sons, 1895.)

Motive Powers and their Practical Selection. By Reginald Bolton, A.M.Inst.C.E. Pp. 250. (London and New York: Longmans, Green, and Co., 1895.)

THE first of these works is a second and enlarged edition of an excellent treatise on a subject seldom dealt with in English text-books. The question of the application of water-power to useful purposes is becoming more and more of importance, and the study of the design and construction of the necessary machinery naturally follows. Continental engineers are in advance of us in this matter, they having long studied the problem successfully. This difference, however, is rapidly disappearing, and will be greatly assisted by the issue of this work.

The author has consulted to a greater or less extent many works and publications, and appears to have brought together much valuable information; this, combined with his own experience, makes the work an important one. Historical matter has been purposely avoided, as well as descriptions of obsolete forms of motors. The author rather jocularly observes in his preface that he is sure to be criticised, one way or the other, as to the use of mathematics in his work. On the question of the free use of mathematical methods we are entirely of his opinion, viz. that formulæ afford the readiest means of accurately stating facts which in the simplest cases can only be verbally defined in elaborate phraseology. The description of the Niagara Falls installation is concise and to the point. This installation is designed for utilising 10,200 cubic feet of water per second, with an available head of 140 feet, which is equivalent, with an assumed efficiency of turbine of 75 per cent. to about 120,000 horse power. The turbines were designed by Messrs. Faesch and Piccard, of Geneva, and made by the I.P. Morris Company of Philadelphia; each of these wheels is to develop 5000-horse power, with a mean head of 136 feet. Other interesting descriptions of recent installations are added, but we miss an account of the Worcester Electric Lighting Station. This is to be regretted, because the installation is an example of a considerable application of water-power under somewhat adverse conditions. The ground covered in this book, both theoretical and practical, is of considerable extent. The author handles the subject in a sensible manner, and arranges it in such a way that the student can have little difficulty in mastering it. For the engineer who looks for theoretical considerations, there is ample food for reflection. The descriptions of the general theory of various turbines are remarkably clear, and are assisted by diagrams and woodcuts. To those engaged in the design of turbines the volume must be invaluable.

Mr. Bolton's book on "Motive Powers" is of a very different nature, and belongs to that large number of text-books written under the impression that a mere stringing together of "facts, formulæ, and data" is of service to the non-technical reader. The choice of a motor for any particular duty, of course, largely depends on various circumstances, and these must be considered by a qualified engineer. It is questionable whether any amount of study can qualify a non-technical man to make a suitable choice in such a matter. The book, however, contains a large store of information suitable for engineers, and it is arranged in a way that easy reference is possible, which is an important consideration. The author very properly falls foul of the term "nominal horse-power," a useless term, and one very likely to mislead. It is quite time that steam and other engines were sold as representing the available power, or "brake horse-power." Under the head of power defined and compared, the author might have been more explicit in his definition of the relation between "the watt" and the "horse-power"; 746 watts are equal to one *electrical* horse-power. In the chapter on the power of the wind, there is an interesting description of an electrical plant for lighting, which was used in London some time ago, the motive power being a windmill on the top of the building. There appears to be an opening for this type of motor. The author gives rules and tables for their design and construction.

Water-wheels, turbines, and hydraulic motors generally come in for a good deal of notice. The information given concerning these motors is very much condensed, but is in a useful form. "Molesworth's Pocket-book" is quoted for rules for the actual construction of turbines; Bodmer's book can be added with advantage. The question of labour and attendance has to be carefully considered in connection with the adoption of steam-power; a type of motor which, for small powers, is being discarded in favour of oil and gas engines. The steam engine, however, has points in its favour, simplicity of parts being not the least of them. The author gives a table showing relative values for heating purposes of various fuels; this is of value, and may prove of use to many steam users.

Under the heading of liquid fuel, no observations are to be found describing "Holden's System" for burning oil, tar, &c.; this should be added in a future edition. An essential feature of this work is a statement of the probable cost of the machinery described, thus rendering a comparison possible of alternate schemes. The condensation of exhaust steam from engines in large towns is an important question, because in some cases it may become a nuisance. The author describes the usual methods in vogue, but omits to mention the atmospheric condenser used to condense the steam, and so get rid of it. Steam engines of various kinds are fully dealt with, including those suitable for dynamo driving. Under the latter class we find no description of the Willan's central valve engine, probably the most efficient of any. If chapter xxi. is intended to include this engine, why not say so?

The author has much to say on the subject of different types of boilers. On page 179 we find a table giving the pitch of stays in flat surfaces in locomotive fire-boxes. This requires considerable alteration. The pitches given for the higher pressures and ½-inch plates are ridiculous; no locomotive builder exceeds 4¼ inches pitch with copper fire-boxes. The usual hydraulic test for boilers is stated to be twice the working pressure. This is so in many cases, and we agree with the author that the boiler is needlessly strained. One and a half times the working pressure is ample, and is quite sufficient to test the workmanship. As to the general essentials for good boiler work, given on page 181, we cordially agree, but would add that machine flanging should, if possible, be done at one heat.

Much has been said of late about the virtues of the tubulous boiler. No doubt its convenience of transport is great, repairs are easily effected, and steam can be rapidly raised. The author gives some interesting data on these boilers, including the Belleville type now being adopted in this country.

Users of small power motors will be interested in chapter xxx. *et seq.* These deal with gas and oil engines, and contain some interesting information. Taken as a whole, this book contains a serviceable collection of data on various subjects. The volume should prove of use to engineers, who will find in it much information relative to motive powers.

N. J. L.

TRAVELS IN TIBET.

Diary of a Journey through Mongolia and Tibet in 1891 *and* 1892. By William Woodville Rockhill. 8vo. Pp. xx. and 414. Illustrations. (Washington: published by the Smithsonian Institution, 1894.)

MR. ROCKHILL is no stranger to the British public; his admirable work on Tibet—"The Land of the Lamas," published in 1891—has been widely read, and his second great journey, described in the book now before us, earned for him the gold medal of the Royal Geographical Society, the highest geographical prize in the world. The book, as now published, differs from "The Land of the Lamas" by being less a piece of literature for general reading than a compendious record of observations suited for serious students of Central Asia.

Tibet is peculiar amongst the regions of the world by possessing almost every possible barrier to discourage the would-be explorer. Its physical conditions, lying in the centre of the largest continent, raised, though just without the tropic, into the frigid zone of altitude, composed in large part of rainless arid plains, girdled by the most stupendous mountains of the earth, conspire with the fanatical exclusiveness of its governing body to keep the land in seclusion. There have been fewer travellers in Tibet than in almost any other area of the known world. In his preface Mr. Rockhill recalls the deeds of his predecessors from Friar Oderic in 1325 to the Russian, French, and British travellers of the last decade. The last Europeans to reach the capital city of Lhasa were the Lazarist fathers, Huc and Gabet, in 1846. Since then the Indian native surveyor, Sarat Chandra Das, has succeeded in disguise in making a survey of the town, but every European has been successfully stopped and turned back at the entrance to Lhasa territory. Mr. Rockhill was no more fortunate in evading this fate than his predecessors were, or than his successor, Miss Annie Taylor, has been; but he was fortunate in being able to give an excellent account of the portions of the country which he visited. Mr. Rockhill has the almost unique attainment of knowing both the Chinese and the Tibetan literary languages perfectly; consequently he was able to make his own negotiations with the natives, and to obtain information from them at first hand. It is gratifying to find that one result of his careful study of Tibet is to vindicate the general truthfulness of the Abbé Huc's picturesque description of the country and the people, which is really responsible for such popular knowledge of Tibet as exists in European literature, and on which some recent travellers, misled by bad interpreters, had cast serious doubts.

Mr. Rockhill describes his journey in the form of a diary, a form which throws all the details into equal and somewhat undue prominence, demanding very careful reading, and many references to different passages, before the general bearing can be understood. A series of appendices containing vocabularies of the Salar, and San-Ch'uan T'u-jen languages, a list of the plants met with, compiled by Mr. W. Botting Hemsley, a table of latitudes and altitudes, and a few meteorological statistics, in some measure makes up for the defects of the diary form. The index, which is all-important in a book of this kind, is unsatisfactory; the entries are numerous enough, but they are not descriptive. The mere facts that snow is referred to on twenty-eight specified pages, and sandstone on forty, does not assist the reader in the way a well-arranged index should. On the other hand, the illustrations are excellent, and leave nothing to be desired, except indeed that they were more numerous.

A map, on the generous scale of thirty-two miles to an inch, gives details of the route, but it is confined to Mr. Rockhill's own surveys, all outside being left blank.

Mr. Rockhill left Pekin in the hope of crossing Tibet from north to south, by a road leading to India, without touching Lhasa territory. He accordingly made his way through Mongolia, passing by Ordos and Alashan, up the valley of the Yellow River to Hsi-ning, and collecting the necessary material for a long desert journey, he left Lusar (Kumbum) on February 17, 1892, passed westward through the marshes of Tsaidam, and at the Naichi Gol, on May 17, turned south-westward with guides who had agreed to take him across the mountains to the Tengri-nor. It was a severe journey: grass for the horses and mules was often scarce; snow fell at midsummer, and herds of wild-yaks and wild-asses were the only living creatures to be seen. The snow-line appeared to be about 17,000 feet, but no glaciers were to be seen on any of the mountains. At length, on July 6, after three days' travelling without food, supporting life only on tea, the party sighted the tents of the Namru Tibetans, about two days' journey from the Tengri-nor. Here there was safety from starvation, but the tribe being under the government of Lhasa, the inevitable result followed. The tribe mustered sixty or eighty armed men, and with the utmost courtesy the head men, reinforced by officials from Lhasa, forbade any advance southward. After much talking, Mr. Rockhill secured the alternative of returning as he came, or going eastward to China *viâ* Ta-chien-lu, which was reached on October 1. By avoiding the high road, Mr. Rockhill succeeded in surveying a good deal of new country, and he made many most interesting observations on the people, who in south-eastern Tibet are much more liberal and enlightened than in the neighbourhood of Lhasa.

On returning to Shanghai the traveller found that in the eleven months since he had left it he had travelled 8000 miles, of which he had surveyed 3400 miles, and crossed 69 passes, all more than 14,500 feet above the sea. Three hundred photographs were taken, and between three and four hundred ethnological specimens collected. The journey was in fact a great and a successful one, though it led to no sensational discoveries; and we believe that the work of the American traveller from the east will bear the closest comparison with that of the Russian explorers from the north, and the British and Indian surveyors from the south.

Hugh Robert Mill.

MIND AND BODY.

The Philosophy of Mind; an Essay in the Metaphysics of Psychology. By G. T. Ladd, Professor of Philosophy in the Yale University. (Longmans, Green, and Co., 1895.)

PROF. LADD'S latest book opens with two excellent chapters on the connection between psychology and the philosophy of mind, which lead one to hope great things of the rest of the work. It is refreshing to find an author deliver an energetic and effective protest against the "water-tight compartment" theory—that science, and even the science of psychology, can get on without metaphysics—and then turn round and declare in favour of a good healthy realism. It is a psychological fact which is well worth keeping in mind, that we all naturally are, and, even in spite of philosophic training, in our ordinary life remain, dualistic realists. This metaphysical position is implied in all the language of science; so that, in particular, it is well-nigh impossible to interpret the results of psycho-physics in any other sense. His arguments against the view of consciousness as a mere series of passive states, which he attributes to Prof. James, are well worthy of attention, and further great expectations will be raised in the mind of the reader by the heading of the fifth chapter—"The consciousness of identity, and so-called double consciousness." For surely it is time that professed psychologists should give up ignoring the alleged facts of multiple personality and the various phenomena connected with "suggestion" and "hypnotism." Whence are we to learn about the psychological import of these things if not from them? But the expectation is unfortunately doomed to disappointment. After making some show of attacking the question, and expressing a pious belief that "the explanation of double-consciousness, when the facts are ascertained and the explanation is made, will be found in extension rather than reversal of the principles already known to apply to the normal activity of body and mind" (p. 168), he "feels obliged for the present to maintain a position of reserve." He admits, indeed, that if an individual should alternate from one condition to another, between which no actual connection by way of self-consciousness, memory, or thought could be traced (and, presumably, *à fortiori*, if both conditions should co-exist and manifest themthemselves by different channels, *e.g.* by speech and so-called "automatic" writing), we should have a true case of "double Ego." But he goes on to declare that "no such case, so far as the evidence is as yet sifted and understood, has ever occurred." It cannot be supposed that a professor of psychology has never come across the evidence; we can, therefore, only suppose that he relies upon the efficacy of his saving clause; for such cases have certainly been reported in abundance, though it may be that the evidence with respect to them is not yet thoroughly "sifted and understood."

The main thesis of the book, however, is the duality of body and mind; or, at least, the defence of natural dualism against such rival theories as Prof. Ladd conceives to be arrayed against it. It may, however, fairly be doubted whether any materialist; spiritualist, or monist would recognise his own theory among the dummies which Prof. Ladd puts up to knock down again.

He admits, in a note, that it is not likely that any one could be found to espouse the cause of what he calls materialism. The most effective answer he has to give to "monistic spiritualism," that if consistently argued out it would lead to solipsism, applies rather to idealism than to the animism against which the rest of his argument is directed. To his polemic against monism it might be objected, as to that against materialism, that no one would be found to defend the views attacked—at least, surely no one who believed, not only in body and mind, but in a third entity also, which is neither (even if this entity is "unknown and unknowable"), could call himself a *Monist*. Monism, as ordinarily understood, is the view, or hypothesis, that the *Träger* of conscious states is just the brain, and nothing else, and conversely that consciousness is a manifestation or aspect of certain brain activities. No third being is required where not even two are postulated. The rest of the argument against monism is to the effect that the supposed psycho-physical parallelism is not completely proven—which may be admitted—and even that in some cases it can be shown not to exist, a point on which Prof. Ladd's arguments hardly seem conclusive. The weakest part of the argument, however, is the implied idea, so common in philosophical discussions, that a metaphysical theory to be accepted ought to be capable of rigid demonstration, instead of being of the nature of an hypothesis postulated to explain the facts of consciousness, which can never be absolutely proved, but may be believed in with greater or less strength of conviction. It is therefore no argument against the monistic hypothesis to say we cannot yet, and probably never will be able to, trace the psycho-physical parallelism everywhere.

The most curious thing in the book remains, however, to be told. In its last pages the author admits not only that "this dualism is not the final word," but that "it must undoubtedly be dissolved in some ultimate monistic solution"! And it must be a little annoying to the monists, whom he has so bitterly attacked, to find that this is a problem which "this treatise hands over to the larger and all-inclusive domain of philosophy."

EDWARD T. DIXON.

OUR BOOK SHELF.

The Story of "Primitive" Man. By Edward Clodd. Pp. 206. (London: George Newnes, Limited, 1895.)

A BOOK such as this forms a useful stepping-stone to higher knowledge; it creates interest, and develops a desire for further information, therefore it possesses the chief qualities that go to make a good book for the average man. For the reader who wishes to know more about the subject than can be compressed in two hundred small pages, a list of books is given at the end of the volume. The illustrations are numerous, but some of these are badly printed. The text is very attractively written, scarcely a sentence being beyond the comprehension of the popular mind. Though the story is briefly told, we have no doubt it will prove interesting to a wide circle of readers. It may be well to point out that the remarks with reference to the chipped flints found in what was believed to be an Upper Miocene deposit in Further India (pp. 23, 24), will need modification when the book comes to a second edition, the bed in which the flints occur having been shown to be Pliocene (see NATURE, vol. li. p. 608).

Britain's Naval Power. By Hamilton Williams. (London: Macmillan, 1894.)

THIS little volume ought to prove very useful to those who wish to know the chief events in the rise of Britain's naval power, without having to plod through details of little consequence. All the great battles are described, and plans of the actions are supplied with them. Celebrated single actions are also mentioned, and although, as the author himself states, some parts require revision and slight corrections, the volume is altogether a light and readable history of the first line of defence, to be commended to every one who desires to know something about naval battles without undertaking a systematic study of the subject.

Portraits berühmter Naturforscher. (Wien und Leipzig: A. Pichler's Witwe and Sohn.)

THE forty-eight portraits which, with short biographical sketches, make up this album, represent well-known men of science of the past and the present. With one or two exceptions, the plates are finely engraved from good portraits. Among our own countrymen in the collection are Darwin, Faraday, Sir William Herschel, Newton, Lord Kelvin (who is given his old and better-known name), and Tyndall.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Discovery of Aboriginal Indian Remains in Jamaica.

THE island of Jamaica, at the time of its discovery by Columbus in 1494, is estimated to have been inhabited by about 600,000 natives, belonging to the race of the Arawâks—a people of simple habits and of a peaceable disposition. The barbarous and cruel treatment of these Indians by their Spanish conquerors, so rapidly decreased their numbers, that in 1655, the date of the conquest of the island by the English, it is probable that not a single specimen of the original type of inhabitant remained alive. Very little was left behind as a record that ever such a race existed here. A few pieces of earthenware showing very primitive ornamentation, and a few flint implements and beads, are practically all that remain to represent their arts and manufactures. Parts of the interior of the country are formed of Miocene limestone, and in this, many caves are to be found. Most of them have, however, yielded little of interest. In one, at Pedro Bluff, the only two aboriginal skulls hitherto known were found. These were submitted to Sir William Flower, and show a frontal compression with corresponding lateral expansion, a deformation produced artifically during infancy by the former inhabitants of the West Indian islands. A kitchen-midden at Northbrook, investigated by Lady Blake, has yielded pieces of ancient pottery, flint implements, shells, and bones of the Jamaica coney, *Capromys brachyurus*, Hill.

Great interest has been aroused in the island within the past few weeks by the discovery of a cave containing the skeletons of at least twenty-four individuals; the ages varying from that of a child with the permanent dentition not yet appearing, to that of aged persons with the teeth-sockets obliterated. Many of the skulls in their depressed frontal region resemble those from Pedro Bluff, and are, no doubt, aboriginal in type. There is, however, considerable variation in the amount of compression. Four of the skulls have been taken to England by Mr. Cundall, the Secretary of the Jamaica Institute, to be submitted to Sir William Flower.

A somewhat shattered canoe, about 7 feet long and 1½ feet wide, made of cedar-wood, was lying above many of the skeletons. An outer portion of the trunk of an *arbor-vitæ*, probably serving at one time as a "mortar," scarcely shows any signs of decay, as a result of the three or four hundred years it may have been in the cave. Among the remains were also obtained the perfect skulls and other parts of the skeleton of two coneys; two large marine shells (*Fusus* and *Murex*), soft parts of which are still eaten by the natives; numerous land shells (*Helix*), and insect remains.

Two small, nearly perfect, earthenware vessels were also found, similar to those known to have been made by the Arawâks. One of these *sappooras* is oval in shape, 7 inches in length and 2 inches high, with a rude handle at each end; the other is round, with a small ledge below the upper margin. Along with these were fragments of pottery belonging to a much larger specimen.

The cave was discovered by the Rev. W. W. Rumsey on the Halberstadt estate belonging to Mr. Gossett. It is in a wild rocky part of the Port Royal Mountains, at a height of about 2000 feet above the sea. The narrow entrance in the face of the hill-side was blocked by boulders of limestone. On removing these, a cavern with waterworn sides, partially covered with stalactitic deposits, was displayed, penetrating into the rock for a distance of about 20 feet, and in some places two or three feet high. The floor is covered with a deposit about 12 inches thick, of a fine, light yellowish dust, but the remains were superficial.

The size of the cave is not such as could possibly contain the whole of the individuals when alive, so that it is probable that it must have been used at one time as a burial-place; while the presence of the canoe, mortar, earthenware, coney bones, marine shells, and a flint implement, is suggestive that some of the people may have lived or fled there for safety, and perhaps been immured by their destroyers, the Spaniards. Whatever may be the explanation of their occurrence, the acquisition of the remains, which have been presented to the Museum, will be a great addition to the archæology of Jamaica.

Museum, Jamaica, May 28. J. E. DUERDEN.

The Antiquity of the Medical Profession.

WITH reference to Mr. H. Spencer's article on the evolution of the medical profession, in the *Contemporary Review* for June, it may be inferred that his remarks should only apply to its historical state in Britain, and not to that in European countries.

It may be pointed out that the profession had existed many centuries before that epoch in the Roman and Grecian nations, as may be seen by any one in looking over Lemprière's Dictionaries.

We have their medical works handed down to us in Celsus (14 A.D.) and Hippocrates (422 B.C.); likewise the Greek army at Troy (1184 B.C.) had military surgeons (Machaon); and Prof. Simpson had discoursed on those in the Roman armies—papers indicated 1856.

See also Dr. Smith's Dictionary, "Greek and Roman Antiquities," for articles on the subjects under:—Art. Medicus, art. Medicina, art. Chirurgia, art. Physiologia.

The art of medicine seems to have been ushered off the stage in the Dark Ages, and to have been consigned to the care of the monasteries and monks for a long period.

It would seem then, from history, that the medical profession is quite as old as either that of theology or law.

Edinburgh, June 17. W. G. BLACK.

A History of British Earthquakes.

ON two or three occasions you have allowed me to ask the readers of NATURE for aid in studying recent British earthquakes, and I have gratefully to acknowledge the valuable assistance which I have thus obtained.

If I might trespass once more upon your space, I should be glad to mention that I am now preparing a history of British earthquakes during the nineteenth century, and would thankfully receive notices of any shocks, either past or future, which your readers may be able and willing to send me. Extracts from provincial newspapers, from private diaries, or from any other trustworthy source, would be most useful.

With a view to aiding in the more careful observation of earthquakes in the future, I have drawn up a short paper of suggestions, and this I shall be happy to send to any one who may wish for it on receipt of his name and address. Those who desire to examine accounts of recent earthquakes in this country, I may refer to the *Proceedings* of the Royal Society for 1894, the *Quarterly Journal* of the Geological Society for 1891, and the *Geological Magazine* for 1891–1893. CHARLES DAVISON.

373, Gillott Road, Birmingham, June 17.

TERMS OF IMPRISONMENT.

IT would have been expected that the various terms of imprisonment awarded by judges should fall into a continuous series. Such, however, is not the case, as is shown by Table I., which is derived from a Parliamentary Blue-book recently published under the title of "Part I.—Criminal Statistics," p. 215. The original has been considerably reduced in size; first, by limiting the extracted data to sentences passed on male prisoners without the option of a fine, and, secondly, by entering the number of sentences to the nearest tenth or hundredth, as stated in the headings to the columns. The material dealt with is thereby more homogeneous than in the original, and its significance is more easily seen. The number of cases is amply sufficient to afford a solid base for broad conclusions, there being in round numbers 830 sentences for various terms of years, 10,540 for various terms of months, and 43,300 for various terms of weeks. The diagram drawn from Table I. gives a still clearer view of the distribution of these sentences:—

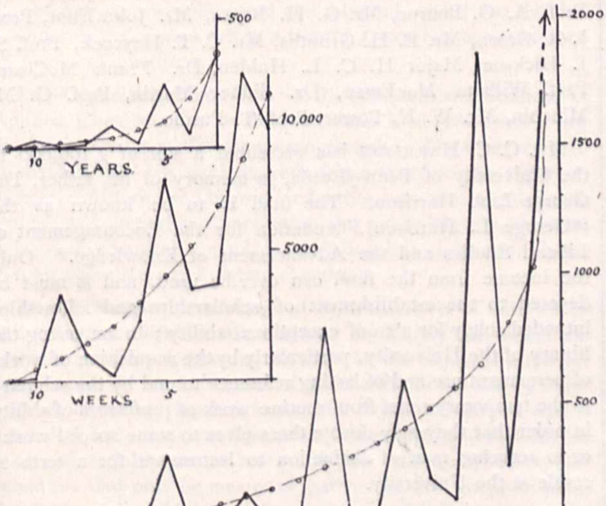
TABLE I.—*Distribution of Sentences.*

Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One hundredth (to nearest integer) of the number of sentences.	
Years.	Recorded.	Smoothed.	Months.	Recorded.	Smoothed.	Months.	Recorded.	Smoothed.	Weeks.	Recorded.	Smoothed.
16–	0		24–	5	1	10–	9	34	11–	0	0
15–	1		23–	0	2	9–	59	40	10–	1	5
14–	1		22–	1	2	8–	21	47	9–	33	9
13–	0		21–	2	3	7–	13	56	8–	10	14
12–	1		20–	3	4	6–	185	65	7–	2	21
11–	0	1	19–	2	5	5–	26	81	6–	23	30
10–	3	1	18–	30	6	4–	112	102	5–	77	40
9–	0	2	17–	0	9	3–	480	480	4–	35	52
8–	1	3	16–	3	12				3–	37	67
7–	8	4	15–	16	14				2–	118	85
6–	2	7	14–	3	17				1–	97	110
5–	24	10	13–	4	20						
4–	6	19	12–	79	25						
3–	36	36	11–	1	29						
	83	83		149	149		905	905		433	433

NOTE.—In reading the table, "16–" means "16 and above 15"; "15—" means "15 and above 14"; &c. The number of these intermediate cases are presumably insignificant; they are not noticed in the diagram, where all cases are referred to the upper of their limiting values.

The extreme irregularity of the frequency of the different terms of imprisonment forces itself on the attention. It is impossible to believe that a judicial system acts fairly, which, when it allots only 20 sentences to 6 years imprisonment, allots as many as 240 to 5 years, as few as 60 to 4 years, and as many as 360 to 3 years. Or that, while there are 20 sentences to 19 months, there should be 300 to 18, none to 17, 30 to 16, and 150 to 15. The terms of weeks are distributed just as irregularly. Runs of figures like these testify to some powerful cause of disturbance which interferes with the orderly distribution of punishment in conformity with penal deserts.

On examining the diagram we are struck with the apparent facility of drawing a smooth curve, that shall cut off as much from the hill-tops of the irregular trace as will fill their adjacent valleys. This has been done, by eye, in the diagram, the small circles indicating the smoothed values. Care has been taken that the sums of the ordinates drawn to the smooth curves should be equal to sums of those drawn to the traces, as is shown by the totals in the bottom line of Table I. The smoothed curves may therefore be accepted as an approximate rendering of the general drift of the intentions of the judges as a whole, and show that the sentences passed



by them severally, ought to be made more appropriate to the penal deserts of the prisoners than they are at present. The steep sweeps of the curves afford a strong testimony to the discriminative capacity of the judges, for if their discrimination had been *nil* and the sentences given at random, those steep curves would be replaced by horizontal lines. We have now to discuss the disturbing cause or causes that stand in the way of appropriate sentences.

The terms of imprisonment that are most frequently awarded, fall into rhythmic series. Beginning with the sentences reckoned in months, we see that their maxima of frequency are at 3, 6, 9, 12, 15, and 18 months, which are separated from one another by the uniform interval of 3 months, or a quarter of a year—a round figure that must commend itself to the judge by its simplicity. And we may in consequence be pretty sure that if the year had happened to be divided into 10 periods instead of 12, the exact equivalent of 3 months, which would then have been $2\frac{1}{2}$ periods, would not have been used in its place. If this supposition be correct, the same penal deserts would have been treated differently to what they are now.

Thus the precise position of the maxima has been apparently determined by numerical fancy, and it seems that the irregularity of the trace is mainly due to the award of sentences being usually in terms of the 3-monthly, but sometimes in that of the 1-monthly, series. The trustworthiness of this solution is tested by grouping the entries in sets of three, each set having one of the maxima for its middle member, as shown in Table II. (where, however, the first and last entries are perforce limited to sets of two). The agreement between the recorded and the smoothed entries is now passably good; it would become somewhat closer if the smoothed curve were revised by paying regard to the series of sets of three, thereby taking facts into account that were not utilised before.

TABLE II. (derived from Table I.).

Terms of sentence in months.	Number of sentences.	
	Recorded.	Smoothed.
24 and 23	5	3
22 — 20	6	9
19 — 17	32	20
16 — 14	22	43
13 — 11	84	74
10 — 8	89	121
7 — 5	224	202
4 and 3	592	582
	1054	1054

This solution does not, however, account for all the peculiarities of the irregular trace. For instance, in the original table in the Blue-book, absolutely not a single sentence of 17 months has been recorded, although there are 32 sentences of 16 months, and 340 of 18. I account for the absence of the number 17, by the undoubted fact that almost all persons have a disposition to dwell upon certain numbers, and an indisposition to use others, and that 17 is one of the latter. These curious whimsies become conspicuous whenever calculators, who are not forewarned, are set to record long series of measures, entering them *by estimation* to the nearest decimal of the divisions of the scale they use. Each figure from 0 to 9, in the decimal place, ought then to occur with equal frequency, but they never do; there is always a run upon some figures, while others are hardly, if ever, introduced. The fancies in this respect of different persons differ widely; the biblical Jews, for example, were fond of 40, apparently employing it as a noun of indefinite multitude, but it has no preferential use with us. On the other hand, it is probable that a large and awkward prime number, such as 17, would be generally in disfavour.

As regards the sentences reckoned in years, they range from 3 years upwards (those between 2 and 3 years being here reckoned as 3 years, while those below 2 years are reckoned, as above, in months). The maxima of frequency in this group are at 3, 5, 7, and 10 years, showing a tendency to a unit of 2 years at first, and then, presumably guided by the habit of decimal notation, to jump from 7 to 10. The bias due to decimal notation is forcibly shown by some entries in the original table which fall outside the limits of Table I. It there appears that 7 sentences were awarded for 20 years, and 6 for 15 years, but absolutely none for the 4 intermediate years, 19, 18, 17, 16. It should be added that there were also 8 sentences for 14 and for 12 years respectively. Had these appeared in Table I., they would have been entered to their nearest tenths, that is as 1 in each case, but I did not care to enlarge the table for the sake of including these, comparatively few, additional cases.

The sentences in terms of weeks have their maxima at 2, 5, and 9, for reasons which I do not as yet understand sufficiently to write about.

The general result is that if the judges were to act on uniform rules, the curve of distribution of terms of sentence would be mainly dependent on two sets of causes only, and would become much smoother in consequence. These are: (1) The distribution of true penal deserts; (2) errors of estimation, which would be distributed about each point in the true curve, according to the ordinary law of frequency of error, and with a modulus that might perhaps be determined.

It would be interesting to tabulate the sentences passed by the several judges since their appointments, to discover their respective peculiarities and personal equations, all who exercise extensive jurisdiction in criminal cases being included under the title of judge. We test the acquirements of youths by repeated examinations, but do not as yet employ the methods of statistics to test the performances of professional men. Examiners, for example, should themselves be tested in this way, and I have a fancy that a discussion of the clinical reports at the various large hospitals might enable a cautious statistician to express with some accuracy the curative capacities of different medical men, in numerical terms. Before putting oneself into the hands of any new professional adviser, it would certainly be a grateful help to know the indexes of capacity of those among whom the choice lay, not merely such as might be inferred from their performances in school and undergraduate days, or by their unchecked professional repute, but as they really are in their mature and practical life.

I will conclude by moralising on the large effects upon the durance of a prisoner, that flow from such irrelevant influences as the associations connected with decimal or duodecimal habits and the unconscious favour or disfavour felt for particular numbers. These trifles have been now shown on fairly trustworthy evidence to determine the choice of such widely different sentences as imprisonment for 3 or 5 years, of 5 or 7, and of 7 or 10, for crimes whose penal deserts would otherwise be rated at 4, 6, and 8 or 9 years respectively. FRANCIS GALTON.

PROFESSOR FRANZ NEUMANN.

AS already announced (p. 133) Prof. Neumann, the eminent physicist and mathematician, died on May 23 at Königsberg at the age of ninety-seven. At a recent meeting of the Paris Academy, the Secretary, M. Bertrand, in announcing the loss the Academy had sustained by the death of such a distinguished Correspondent in the Geometry Section, pronounced the following short *éloge* on Prof. Neumann's contributions to knowledge:—

"Franz Neumann, Professor of Physics and Mineralogy at the University of Königsberg, made his *début* in science more than seventy years ago, by some beautiful works on mineralogy. Soon after he directed his studies towards physics, and by an admirable 'Mémoire sur la Théorie des Ondulations,' which was presented to the Berlin Academy in 1835, he took his place among the masters of science. Neumann, like Cauchy, but by very different means, was led to consider luminous vibrations as taking place in the plane of polarisation, while Fresnel thought them perpendicular; he knew how to follow in the most minute details, always in accordance with the observation, the mathematical consequences of his hypothesis. But Fresnel's theory is not contradicted by any of the experiments, so doubt continues, and the ever renewed discussions, whatever their conclusion may be, will remain a noble homage to the man of science and profound physicist who was the first to start them.

"Neumann's memoir on induction showed again the great mathematical skill of its author. In it Neumann translated, by general formulæ, the discoveries of Faraday and Lenz's laws; it is to him that we owe the expression of the potential of a system of two closed currents, of which merely the existence, independently of the very elegant form which he has given it, has played such a great part in science.

"Franz Neumann was a great Professor. Even at the age of ninety he attracted numerous auditors; his lessons, received and written out by learned students, have been studied in all the universities of Europe. The study of physics was his aim; but when he came across a fine mathematical problem, he excelled in interesting his auditors by initiating them occasionally into the highest theories of analysis. It is with justice that in 1863 the Section of Geometry, making amends for a long neglect, elected this illustrious physicist into the Academy."

NOTES.

THE annual meeting of the Royal Society for the election of Fellows was held on Thursday last, when the following gentlemen were elected into the Society:—Mr. J. Wolfe Barry, C.B., Prof. A. G. Bourne, Mr. G. H. Bryan, Mr. John Eliot, Prof. J. R. Green, Mr. E. H. Griffiths, Mr. C. T. Heycock, Prof. S. J. Hickson, Major H. C. L. Holden, Dr. Frank McClean, Prof. William MacEwen, Dr. Sidney Martin, Prof. G. M. Minchin, Mr. W. H. Power, Prof. T. Purdie.

MR. C. C. HARRISON has presented a sum of £100,000 to the University of Pennsylvania, in memory of his father, Dr. George Lieb Harrison. The fund is to be known as the "George L. Harrison Foundation for the Encouragement of Liberal Studies and the Advancement of Knowledge." Only the income from the fund can ever be used, and it must be devoted to the establishment of scholarships and fellowships intended solely for men of exceptional ability; to increasing the library of the University, particularly by the acquisition of works of permanent use and of lasting reference to and by the scholar; to the temporary relief from routine work of professors of ability in order that they may devote themselves to some special work; or to securing men of distinction to lecture and for a term to reside at the University.

Science gives the following as the preliminary arrangements for the forty-fourth meeting of the American Association for the Advancement of Science, to be held in Springfield, Mass., from August 28 to September 7, 1895:—At the first general session the President-elect, Prof. E. W. Morley, will be introduced by the retiring President, Prof. D. G. Brinton, who will afterwards give an address on "The Aims of Anthropology." The Presidents of the sections, and the subjects of some of their addresses, are as follows:—Section of Physics: "The Problem of Aerial Locomotion," W. Le Conte Stevens. Section of Anthropology: F. H. Cushing. Section of Geology and Geography: "The Geological Survey of Virginia, 1835–1841—its History and Influence in the Advancement of Geologic Science," Jed. Hotchkiss. Section of Economic Science and Statistics: "The Providential Function of Government in Relation to Natural Resources," B. E. Fernow. Section of Chemistry: McMurtie. Section of Botany: "The Development of Vegetable Physiology," J. C. Arthur. Section of Mechanical Science and Engineering: William Kent. The affiliated societies meeting in conjunction with the Association are:—The Geological Society of America: Prof. N. S. Shaler, President; Prof. H. L. Fairchild, Secretary. Society for Promotion of Agricultural Science: Prof. William Saunders, President; Prof. William Frear, Secretary. Association of Economic Entomologists. Association of State Weather Service: Major H. H. C. Dunwoody, President; James Berry, Secretary. Society for Promoting Engineering Education: Geo. F. Swain, President; Prof. J. B. Johnson, Secretary. American Chemical Society: Edgar F. Smith, President; Prof. Albert C. Hale, Secretary. American Forestry Association: Hon. J. Sterling Morton, President; F. H. Newell, Secretary. Applications relating to membership and papers should be sent to Prof. F. W. Putnam, Permanent Secretary, Salem, Mass. For all matters relating to local arrangements, hotels, railway rates and certificates, Mr. W. A. Webster, Local Secretary, A. A. A. S., Springfield, Mass., should be addressed.

MR. R. F. STUPART has succeeded the late Mr. C. Carpmael, as Director of the Meteorological Service of Canada.

THE Grocers' Company have renewed the research scholarship held by Mr. Leonard Hill, and have elected Dr. J. Haldane and Prof. Waymouth Reid to the places vacated by Dr. Vaughan Harley and Dr. E. Stirling. The scholarships are each of the value of £250 a year.

AT the annual meeting of the London Library, held on Friday last, Mr. Herbert Spencer was elected a vice-president, and Prof. Huxley was elected a member of the committee. A scheme for the reconstruction and extension of the premises, at an estimated cost of £17,000, was discussed and adopted, and it was decided to commence the work when a sum of £5000 has been obtained by means of donations.

THE Organising Committee of the International Congress of Applied Chemistry, to be held in Paris next year, met a few days ago to make preliminary arrangements. The Congress will be divided into ten sections, referring respectively to sugar refineries, distilleries and brewing industries, agricultural industries, agricultural chemistry, alimentation and public hygiene, chemical industries, chemical apparatus, metallurgical chemistry, photographic chemistry, and electro-chemistry.

THE fifth annual conference of representatives of authorities under the Sea Fisheries Act was held on Friday last, under the presidency of Sir Courtenay Boyle. In the course of a few remarks upon the establishment of hatcheries for sea-fish by committees, or out of Imperial funds, Mr. Bryce pointed out that a great deal had been done by marine laboratories and stations for observation, to determine more fully the habits of the fish, and remarked that only by means of hatcheries, and by prohibiting the taking of undersized fish, was it possible to recreate the diminishing supply of our soles and other flat fish.

WE notice with regret that Dr. Valentine Ball, C.B., F.R.S., Director of the National Museum, Dublin, died on Saturday, after a short illness. Dr. Ball was for seventeen years connected with the Geological Survey of India. On the resignation of the chair of Geology in the University of Dublin by Dr. Haughton, he was appointed to it, and twelve years ago he accepted the position which he held at the time of his death. He was the author of several valuable treatises, and while Director of the National Museum, he greatly added to the value of the collections.

SEVERAL exhibitions and congresses of scientific interest are noted in the *Board of Trade Journal* as having been lately projected. In connection with the thirteenth International Exhibition to be held at Bordeaux in September next, the Société Philomathique of the town will organise a congress of technical, industrial, and commercial instruction similar to that held in 1886, at which the English Government was officially represented. An international exhibition of articles of food, clothing, hygienic appliances, sport, and inventions of all kinds will be held at the "Parkhaus," Bremen, in the course of this year. It will be open from September 14 to October 6. An international exhibition will also be held in Montreal, Canada, next year. The exhibition will open in May, and close in October. It will be held on the site of the present exhibition grounds and on adjoining land of the Mount Royal Park, embracing altogether about 120 acres. The buildings will be twenty-seven in number, and will be devoted to fine arts, manufactures, and liberal arts, electricity, machinery, fisheries, forestry, horticulture, agriculture, &c. Finally, according to latest advices from Denver, the plans for the holding of a mining and industrial exhibition in that city, in the fall of next year, are being advanced with vigour and success.

THE New York State Bridge Commission have approved the plan of Engineer Charles MacDonald for a steel suspension bridge from New Jersey to New York City. The bridge will be 5600 feet long, with a length of 3110 feet between piers; 125 feet wide, with room for six railroad tracks; and 150 feet above mean tide-water. The piers will be 557 feet high, supported by 125 feet of solid masonry. The cost is guaranteed not to exceed 25,000,000 dollars. The bridge will be much the largest suspension bridge ever attempted.

ONE of the most remarkable features of earthquake-pulsations is their great duration. The originating earthquake may last but a few seconds or minutes, while the ground at a distance may rock gently through a very small angle for several or many hours. Dr. E. Oddone, of the geodynamic observatory at Pavia, has recently contributed an interesting paper on this subject (*Rend. della R. Acc. dei Lincei*, iv., 1895, pp. 425-430). Making use of the records of distant earthquakes during the years 1893-94 by delicate seismometrographs at Rocca di Papa, Rome and Siena, he arrives at the important conclusion that the duration of the pulsations increases with the distance from the epicentre.

SOME singular curves showing the distribution of daily wind velocities in the United States, are published by Mr. F. Waldo in the current number of the *American Journal of Science*. The stations chosen range from the Atlantic to the Pacific and Mexican coasts, and include Block Island, New York, Cleveland, San Francisco, San Diego, North Platte, Fort Apache, Salt Lake City, and Roseburgh, among others. The months of January and July are selected as typical months for average daily variation. The daily variations are always greater in summer than in winter, except for Fort Apache, on the great plateau, where the excursions are about equal. At this place the velocities vary from 9·2 to 3·3 miles per hour in January, and from 10·1 to 2·9 miles per hour in July, the maximum in each case taking place at about 4 p.m., and the minimum at 8 a.m. The greatest variation of all is shown by the San Francisco curve for July. About 4 p.m. the wind blows with a speed of some 18 miles per hour, which falls to 7 miles per hour in the forenoon. Tatoosh Island shows a minimum at 2 p.m. in January, but its variations in July are similar to those at Block Island in the Atlantic, which shows the same sequence as the continental stations referred to, but with smaller amplitudes.

TWO observations recorded by Mr. W. C. J. Butterfield, in the *Zoologist*, give support to the view that individual female Cuckoos only introduce their eggs into the nests of one particular species of birds, and not indiscriminately into those of any of the birds usually selected as foster-parents. Mr. Butterfield took a Cuckoo's egg from a Wren's nest in the early part of May, and three weeks later found another Wren's nest within a few yards of the former one, also containing a Cuckoo's egg. The two eggs were exactly alike, both as to size, and as to the manner in which the colouring matters and markings were disposed. It is therefore most probable that the eggs were laid by the same bird; for it is well known that a strong family likeness exists between the eggs laid by the same individual, although the eggs of different individuals of the same species may vary considerably. The observation thus affords another instance of a Cuckoo placing its egg in the nest of a particular species of bird, although there were numerous nests of Hedge-Sparrows and other dupes of the bird in the vicinity, into which the egg could have been put with much less difficulty.

A STORY to the effect that a new breed of cats had been produced in the cold-storage warehouses of Pittsburg went the rounds of the newspapers some months ago, and was reprinted in most of our scientific contemporaries. It has even found its way into Mr. Lydekker's recent volume on "Cats." A letter received from the Secretary of the Cold Storage Co., and published in the June number of the *American Naturalist*, shows that the story has but a slight foundation in fact. The letter reads as follows:—"While there is some foundation for the newspaper article, it is somewhat exaggerated. Our cold-storage house is separated into rooms of various sizes, varying from 10° to 40° above zero. About a year ago we discovered mice in one of the rooms of the cold-storage house. We removed one of the cats from the general warehouse to the room referred to in the cold-storage house. While there, she had a litter of several kittens; four of these were transferred into one of the general warehouses, leaving three in the cold-storage house. After the kittens were old enough to take care of themselves, we put the old cat back into the house we had taken her from. The change of climate or temperature seemed to affect her almost immediately. She got very weak and languid. We placed her again in the cold-storage room, when she immediately revived. While the feelers of the cats in the cold-storage room are of the usual length, the fur is thick and the cats are larger, stronger, and healthier than the cats in any of the other warehouses." Thus, it is pointed out, the only result of the change of environment was the usual one which ensues on the advent of winter in extra-tropical latitudes generally.

HERR H. SCHINZ reprints from Engler's *Botanisches Jahrbuch*, vol. xxi., a synopsis of the African Amaranthaceæ, in which a number of new species are described.

THE most recent part published (No. 7) of Dr. George King's "Materials for a Flora of the Malayan Peninsula," published in the *Journal* of the Asiatic Society of Bengal, is occupied by the orders *Meliaceæ*, *Olacineæ*, and *Ilicineæ*. A large number of new species are described, and a new genus, *Bracea*, belonging to the Olacineæ.

IN an article reprinted from the *Ann. de la Société belge de Microscopie*, M. E. Marchal discusses the microbiological processes which take part in the ripening of soft cheeses, especially those known as "fromage de Herve" and "fromage Casette." While a large number of microbes appear to assist in the process, he states that the essential part is played by the fungus known as *Oospora lactis*, Sacc.

IN a previous note (vol. li. p. 540), we have given a brief account of the Vicentini microseismograph erected in the University of Siena. A full description of the instrument, illustrated with three figures, has now been published by the inventor (*Bull. Soc. Veneto-Trentina di Sci. Nat.* vi., 1895), and well deserves the attention of seismologists.

WE are glad to observe that the South London Entomological and Natural History Society reports a prosperous condition, in the volume of *Proceedings* for the year 1894. The Society dates back to 1872, and has been a centre of scientific energies ever since its foundation.

THE papers read at the fifth annual meeting of the Museums Association, held at Dublin a year ago, have just been published in a report of the proceedings at the meeting. The report, which is edited by Mr. E. Howarth and Mr. H. M. Platnauer, should be in the hands of all curators of museums.

THE first number of a bimonthly journal for sanitary engineers will be published at Brussels on August 1, under the title *La Technologie Sanitaire*. It will be under the direction of an editorial committee, the secretary of which is M. Victor J. Van Lint, 115 rue Joseph II., Bruxelles. The journal will deal with all questions relating to public health.

A FULL abstract of a paper on "The Psychologic Development of Medicine," read by Dr. J. H. McCormick before the Johns Hopkins Hospital Historical Club, on April 8, appears in the *Johns Hopkins Hospital Bulletin*, No. 49. The paper follows almost exactly the same lines as Mr. Herbert Spencer's paper in the current number of the *Contemporary Review*.

THE latest addition to the Encyclopédic Scientifique des Aide-Mémoire is "Transmissions par Cables Métalliques," by M. M. H. Léauté and A. Bérard. The transmission of power by metallic cables has given rise to important mathematical developments which are considered in this Aide-Mémoire. The authors confine themselves to the theoretical points which ought to be known to every engineer concerned with cable transmission.

To the series of Economic Classics in course of publication by Messrs. Macmillan, has just been added Thomas Mun's important treatise, "England's Treasure by Forraign Trade," written about 1630, and printed for the first time in 1664. The treatise marks an important period in the history of economic thought, and its author is regarded by political economists as the founder of the mercantile system. In the present reprint of the first edition of the book, the title-page is reproduced in facsimile, and the original spelling and punctuation are followed throughout.

THE third part of "Phycological Memoirs," edited by Mr. George Murray, has just been published by Messrs. Dulau and Co. The memoirs are devoted to researches made in the Botanical Department of the British Museum (Natural History), and the present part contains papers on "A New Part of *Pachytheca*," "Calcareous Pebbles formed by Algæ," "The Sori of *Macrocystis* and *Postelsia*," and "A Comparison of the Arctic and Antarctic Marine Floras." Four very fine lithographed plates illustrate the papers.

THE colours exhibited by the artificial spectrum-top, described and discussed in these columns some months ago, are shown much more distinctly, and in greater variety, by a "Betts's Chromoscope," sent to us by Messrs. George Philip and Son. The instrument consists of an ingenious whirling table, by means of which heart-shaped pieces of cardboard, having arcs of different thicknesses variously disposed upon them, are put in rotation. A moderate speed of rotation produces a very definite impression of coloured rings, and when some of the more complicated designs are used, secondary tints are clearly seen.

MESSRS. J. AND A. CHURCHILL have published an eighth edition of the well-known "Bloxam's Chemistry, Inorganic and Organic," rewritten and revised by Prof. J. M. Thomson and Mr. A. G. Bloxam. Several new woodcuts have been added, and some obsolete ones have been omitted. Considerable changes have been made in the arrangement of the subject-matter, and a large portion of the book has been rewritten, while the whole of it has been well revised. The changes all appear to have been in the direction of improvement; hence the book will hold its place as a good text-book and a handy work of reference.

WE have received from Dr. L. Palazzo an account of a meteorological station recently attached to the laboratories of the Public Health Department in Rome. The authorities, recognising the important connection between various diseases and atmospheric conditions, have provided the station with a full set of instruments, and intend to instruct students belonging to the school annexed to the laboratories in their use, and to include, among other studies, a short course of meteorology as applied to hygiene. The results of the observations will be regularly published in a special bulletin, with a view to determining more particularly the medico-climatology of that city.

MISS E. A. ORMEROD has sent us a leaflet referring to the Forest Fly (*Hippobosca equina*, Linn.), a well-known trouble in the New Forest of Hampshire and its neighbourhood. This fly is to be found on various kinds of animals, as horses, donkeys, cattle, dogs, and cats, to all of which its presence in the hair is a severe annoyance. According to general belief, the fly feeds by blood-sucking; it is also said to find nourishment in the perspiration given off by cattle, but further investigation as to how far this occurs is required. The method adopted to prevent the attacks is to wipe the horses over with a cloth moistened with paraffin, or with some dilute sanitary solution.

WE have received a copy of Mr. W. E. Plummer's Report of the Observations made, under his direction, at the Liverpool Observatory, Birkenhead, during 1894. From observations of twenty-two stars, the latitude of the Observatory, for the mean epoch 1894·7, was found to be 54° 24′ 4″·8. A new longitude determination has also been made; exchange of signals with Greenwich Observatory on thirteen nights gave the value 12m. 17·33s. West of Greenwich. The long series of photograph records accumulated at the Observatory, has been used by Mr. Plummer for the derivation of the diurnal inequality of barometric pressure. The results of his investigation are stated in an appendix to the Report, and are clearly exhibited by means of curves representing the diurnal changes of the barometer in each month, and also for the year.

ONE after another, scientific societies are beginning to organise their literature. Quite recently, under the title "Bibliotheca Geographica," the first volume of a geographical bibliography has been published by the Berlin Gesellschaft für Erdkunde. The volume contains the titles of all the geographical publications during 1891 and 1892, classified into subjects, and each section arranged alphabetically according to the author's names. In general geography there are different classes for text-books, historical geography, mathematical and physical geography, biological geography, and anthropological geography (which covers colonisation and the distribution of disease). The classification adopted for purely geographical papers is very elaborate, and the work done in any region during the years covered by the bibliography can be very easily found. It is proposed to issue annual bibliographies similar to the present volume. The editor of the series is Herr Otto Baschin, and the first volume has been prepared with the assistance of Dr. Ernst Wagner.

THE Belgian Society of Geology, Palæontology and Hydrology, aided by Government and other subsidies, has published the first part of an elaborate rainfall investigation of that country, prepared by A. Lancaster, of the Royal Observatory of Brussels. The author is well known to men of science by various valuable publications, and it was entirely due to his efforts that the rainfall service in its present complete form was established in the year 1882. The complete publication will consist of two or three volumes, the first of which contains 224 octavo pages, accompanied by a map drawn by the Military Cartographical Institute, to the 400,000th of the true scale. The number of rainfall stations dealt with is 282, and the monthly sums and means are given for the whole period, together with a series of tables showing the geographical distribution according to catchment basins, and tinted charts showing various annual rainfall zones. The second part will contain various supplementary tables, such as the distribution of rainfall according to seasons, variability of rainfall, &c.; the expense of this part is to be defrayed from the proceeds of the sale of the first part, which is issued at cost price.

FROM the point of view of stereochemistry, the supposed impossibility of preparing optically active halogen compounds from the corresponding active hydroxy-acids has been a serious defect in the strong array of evidence which has compelled the acceptance of van't Hoff's hypothesis of the asymmetric carbon atom. This defect has at last been remedied by P. Walden, who describes a series of active halogen substitution products in the current number of the *Berichte*. Inquiring whether the inactivity of the halogen derivatives prepared by replacement of the hydroxyl group in active compounds by bromine or by chlorine, were due to an inherent quality of the halogen atom, or rather due to the racemisation of the compounds under the conditions hitherto employed in their production, the author undertook the task of examining the methods used in preparing these compounds. Working on the active hydroxy-acids: malic, tartaric, sarcolactic, and mandelic acids, the substitution of chlorine and bromine for hydroxyl was accomplished by means of phosphorus pentachloride and pentabromide respectively Under the conditions detailed by the author, this substitution was carried out without the racemisation which appears hitherto to have always occurred when these halogen derivatives have been prepared. He has shown that (1) dextro-rotatory chlor- or brom-succinic acid may be prepared from the ordinary lævo-rotatory malic acid; (2) lævo-rotatory tartaric acid yields lævo-rotatory derivatives of its esters, containing a halogen atom in place of a hydroxyl group, which retain the optical activity due to the presence of the asymmetric carbon atom; (3) similarly, dextro-rotatory derivatives of α-chloropropionic acid and α-bromopropionic acid can be obtained from the lævo-rotatory sarcolactic acid; and (4) lævo-rotatory mandelic acid (from amygdalin) yields dextro-rotatory phenylchloracetic acid and phenylbromacetic acid. These active compounds have hitherto only been prepared in the racemic form. Their observed inactivity when so prepared was not due to any accidental limitation of the generality of van't Hoff's theory, but only to the racemisation they had undergone during the process of preparation. It is probably quite generally possible to substitute halogen atoms for hydroxyl groups in combination with active asymmetric carbon atoms without destruction of their optical activity. The activity of the compound depends only on the fact of four different atoms or atomic groups being connected with one and the same carbon atom, while the amount and direction of the rotation produced is unquestionably related to the specific nature of these atoms and groups.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*, ♀ ♀) from India, presented respectively by Mr. Charles Roberts and Miss Wieldt; a Leopard (*Felis pardus*, ♀) from India, presented by Mr. Edward Langworthy; a Common Otter (*Lutra vulgaris*, ♂), British, presented by Mr. M. P. Clarke; a Northern Mocking Bird (*Mimus polyglottus*) from North America, presented by Mr. Henry J. Fulljames; a Yellow-throated Sparrow (*Gymnorhinus flavicollis*), a Double-banded Pigeon (*Treron bicincta*), two Chinese Quails (*Coturnix chinensis*), two White-breasted Gallinules (*Gallinula phœnicura*) from India, presented by Mr. Frank Finn; two Weka Rails (*Ocydromus australis*) from New Zealand, presented by Mr. Reginald Moorhouse; two Southern River Hogs (*Potomachœrus africanus*, ♂ ♀) from East Africa, presented by the late Mr. B. Ward; a European Pond Tortoise (*Emys europæa*), European, presented by Miss Laura Bedford; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Jamaica, presented by Lady Blake; a Black-spotted Teguexin (*Tupinambis nigro-punctatus*) from South America, deposited; a Ring-tailed Phalanger (*Pseudo-*

chirus peregrinus) from Australia, two Nicobar Pigeons (*Calœnas nicobarica*) from the Indian Archipelago, purchased; a Reticulated Python (*Python reticulata*) from Malacca, received in exchange; a Thar (*Capra jemlaica*, ♀), a Red Deer (*Cervus elaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OCCULTATION OF REGULUS.—On June 26 there will be an occultation of Regulus, magnitude 1·5. The disappearance will take place at 8.4 p.m., while the sun is still above the horizon, and the star will reappear at 8.56—that is, about 37 minutes after sunset at Greenwich. The point of disappearance will be at an angle of 147° from the north point towards the east, and of reappearance at 275° reckoned in the same direction. The age of the moon will be a little less than 4 days.

THE RECURRENCE OF ECLIPSES.—A new period of the recurrence of eclipses, which promises to be of great use in the discussion of ancient eclipses, has been investigated by Prof. J. M. Stockwell. (*Astronomical Journal*, No. 346.) He points out that 372 tropical years are very nearly equal to 4601 lunations, and also very nearly equal to twenty revolutions of the moon's node; thus:

372 tropical years	=	135870·10348 days.
4601 lunations	=	135870·23425 ,,
20 revolutions of node	=	135870·700 ,,

During this period, the change of mean longitude of the sun and moon at the time of new moon is $-5°·057$, of the longitude of the moon's perigee $+11°·464$, and of the longitude of the ascending node $+0°·021$. The precession of the equinoxes during 4601 lunations amounts to 5°·1368, so that the mean longitude of the sun and moon when referred to the movable equinox only changes by 0°·0797 in a period of 372 years. From this it follows that if an eclipse happened on a given day of the tropical year, there would be another eclipse on the same day of the tropical year 372 years afterwards.

As an example of the application of this new cycle, Prof. Stockwell gives particulars of an inquiry into an eclipse of the sun which is said to have been observed in China on the day of the autumnal equinox during the twenty-second century B.C. According to Oppolzer, an eclipse occurred at the autumnal equinox in the year B.C. 1039, October 3, and going back three periods of 372 years, the year 2155 B.C. is deduced; other eclipses about this time are found by adding multiples of nineteen years to that date. The discussion of the conditions shows that the eclipse which satisfies the tradition occurred on October 10, 2136 B.C.; this would be visible as a partial eclipse over nearly the whole of China. According to a well-known story, the astronomers Ho and Hi were put to death for having failed to predict this eclipse.

VARIABILITY OF NEBULÆ.—One of the best authenticated cases of a variable nebula is that discovered by Hind in 1852 in the constellation Taurus. The nebula was then easily seen in ordinary telescopes, but D'Arrest was quite unable to see it in October 1861, though it was detected shortly after as an exceedingly faint object in the Pulkowa refractor, and in the following year was seen a little brighter with the same telescope. In 1868, however, the nebula was invisible to Struve, but another nebula was discovered 4' preceding. Struve's nebula was subsequently observed by D'Arrest, who testified to its absence in previous observations of the neighbourhood; it was seen also by Tempel in November 1877, but was not visible to him a month later. The interest attaching to this region was increased when, in 1890, Mr. Burnham found that τ Tauri was involved in nebulosity; this was confirmed by Prof. Barnard, who also observed that Hind's nebula was only just visible with the Lick telescope, while Struve's nebula was not perceptible. In a paper recently communicated to the Royal Astronomical Society, Prof. Barnard states that on February 25 of the present year he found Hind's nebula to be an easy object, while Struve's nebula was absent, and the nebulosity round τ Tauri had practically faded to invisibility. Further observations on March 24 showed that Hind's nebula was again scarcely visible, while τ Tauri was distinctly nebulous, and a faint nebula was suspected in the position assigned to Struve's nebula (*Observatory*, June).

It thus appears that there are really three variable nebulæ in this region, and the observations rather suggest that there is a connection between them. In 1890, Prof. Keeler found that the nebulosity round τ Tauri was probably of the bright-line type, but nothing seems to be at present known as to the spectra of Hind's and Struve's nebulæ. On the meteoritic hypothesis, changes in the brightness of nebulæ are due to the interpenetration of nebulous streams and sheets.

THE ZI-KA-WEI OBSERVATORY.—The Zi-ka-wei (or Sicawei) Observatory, near Shanghai, was founded in 1873 by the French Roman Catholic Mission of Kiang-nan, and provided with the instruments necessary for the study of meteorology and terrestrial magnetism. Since that time, excellent service to commerce and to science has been rendered by the Observatory, by the daily publication of weather bulletins, and the issue of a number of important memoirs. Up to the present, however, astronomy has received little attention at Zi-ka-wei. Twelve years ago, the Municipal Council of the French Settlement furnished the Observatory with a small transit instrument for time determinations in connection with the time-ball service then established, but that instrument represents the whole astronomical outfit. Recognising this deficiency, Father Chevalier, the Director of the Observatory, has made an appeal for funds to purchase a good equatorial telescope. The English Settlement at Shanghai has voted a sum of £400 towards the cost of the instrument, and the French Settlement has granted a like amount. The shipping companies at Shanghai have also promised a sum of about £400, so that £1200 may be taken to be already available. But Father Chevalier wishes to have an instrument with an aperture of about twenty inches, and for this the money already subscribed is insufficient. He has therefore appealed to friends of science in France, America, and England for a sum of about £1000 more. If this is contributed, he hopes to have erected a great equatorial, and to accomplish valuable work with it.

THE ROYAL SOCIETY CONVERSAZIONE.

THE rooms of the Royal Society at Burlington House were filled last Wednesday evening, when the annual conversazione to which ladies are admitted took place. Some of the exhibits were shown at the conversazione on May 1, and have already been described in these columns. Following our usual custom, we only give descriptions of new exhibits.

Perhaps the most striking feature of the evening was the telephonic communication with Edinburgh, Glasgow, Belfast, and Dublin, practically shown by the Postmaster-General. The line used is the first link of the great Trunk Telephone System erected by the Post Office, which will eventually place the chief towns in the British Isles in direct communication with each other. The wires to Ireland extend through Leeds and Carlisle to Portpatrick, thence by cable across the North Channel to Donaghadee, and thence to Belfast and Dublin, the distance by this route from London to Dublin being 467 miles. The lines are so carefully laid that it was easy to converse with persons at the places connected by them, without being disturbed by the foreign sounds usually associated with telephonic communications.

An electograph for indelible linen marking was shown by Messrs. Nalder Bros. and Co. The instrument is used as follows: the fabric is damped and a current is passed for about two seconds from a silver die, carrying silver into the fabric wherever the die touches. The current is then reversed for three seconds, which reduces the silver in the fabric; the final result being the same as with ordinary marking-ink, viz. that metallic silver is deposited in the tissue. Plain water can be used, but a salt solution is preferable, as the result is much more quickly obtained.

Models illustrating Lewis and Hunter's patent coal shipping system, as in use at the Bute Docks, Cardiff, were exhibited by the Bute Docks Company. With this system the coal is shipped in very much better condition than with the old systems, and owing to the construction of the carrying-boxes, with a cone valve or bottom, which is only released to let the load out when it is lowered down into the hold of the vessel, within some 18 inches of the flooring of the ship or the cargo, as the case may be, the breakage is greatly reduced. Each crane is capable of loading 300 tons per hour.

Prof. C. V. Boys illustrated the projection of ripples, and showed a logarithmic chart of wave and ripple velocities and frequencies. Ripples produced by tuning-forks are so small, and travel so quickly, as to be invisible unless illuminated either instantaneously or intermittently at the proper rate. They are then visible, and the relations of velocity and frequency can be illustrated. Both tuning-forks and a mechanical device were employed to produce the ripples. By the use of "scale lines," the logarithmic chart was made more comprehensive than usual. The lines were employed to illustrate the effect of all possible variations of gravity and of surface tension divided by density upon velocities and frequencies of waves and ripples.

Mr. J. Norman Lockyer, C.B., had three exhibits. One was a photograph of apparatus employed for collecting the gases obtained from minerals by the distillation method. A small retort containing the mineral is connected with an end-on spectrum tube joined on to a Sprengel pump. After exhaustion, the mineral is heated to redness, and the spectra of the gases evolved at the various stages, as exhibited by the spectrum tube, are both observed and photographed. The gases are collected in a "steeple" at the foot of the fall tube of the pump, and they can then be observed at atmospheric pressure. The second exhibit consisted of photographs of the spectra of Bellatrix, and of a part of the solar chromosphere, showing coincidences with the lines photographed in the spectra of the gases obtained from uraninite. The photographs showed a close relation of the new gas or gases to solar and stellar phenomena. They appear to point to the *vera causa*, not of two or three, but of many of the lines which so far have been classed as "unknown." The spectrum of Bellatrix was photographed at South Kensington with a 6-inch prism of 45°, and that of the solar chromosphere with the same instrument during the total eclipse of the sun, 1893. Mr. Lockyer also exhibited photographs of the spectra of the new gases. In the preliminary experiments, the new gases have not been separated from the known gases which come over with them, so that the spectra exhibited contained many known lines. The photographs illustrated: (*a*) The presence of the yellow line (D_3) in some instances *with* the blue line 4471, and in others *without* it. (*b*) The presence of the yellow line in some spectra *with* an ultra-violet line at 3889, and in others *without* it.

Dr. A. A. Common exhibited the following silvered glass mirrors: (1) 21-inch convex mirror, 54-inch radius, being the small mirror of an oblique Cassegrain reflecting telescope. (2) 20-inch concave mirror, 90-inch radius, spherical curve. (3) Two 16-inch plane mirrors for heliostats to be used at the 1896 total solar eclipse.

Mr. A. E. Tutton exhibited an instrument for cutting, grinding, and polishing accurately orientated plates and prisms of crystals of every degree of hardness. The instrument combines an accurate reflecting goniometer with a diamond-edged cutting disc and grinding and polishing laps. The adjusting segments of the goniometer are graduated, in order that the crystal may be adjusted so that the desired direction in it can immediately be brought parallel to the cutting disc or grinding lap. Numerous interchangeable laps are provided suitable for all classes of crystals, and the interchange may be effected with great readiness. A counterpoising arrangement is also provided which enables the pressure with which the crystal bears upon the lap to be nicely adjusted, according to the strength of the crystal. The instrument may either be driven by hand or by means of any form of small motor.

Mr. A. P. Trotter showed a model illustrating the relation of volts, amperes, and length of electric arc. The model was made from the diagrams in Prof. Ayrton's paper, read before the Chicago Congress, and described in Mrs. Ayrton's article in *The Electrician*. Drawings of the electric arc were shown by Mrs. Ayrton. The drawings were in sepia, and ten times the full size. They showed the form of the arc produced with a current of 20 amperes between a positive carbon 18 millimetres in diameter, and a negative one 15 millimetres in diameter, when the arc was respectively 4, 7 and 18 millimetres long. From the drawings it could be seen that using a *cored* positive carbon *diminishes* the visible part of the arc, and, when the arc is long, causes the central portion to become gourd-shaped.

The Applied Mathematics Department of University College showed a series of diagrams, calculated and prepared by Miss Alice Lee, to illustrate the time-decay of the field due to a Hertzian oscillator. The late Prof. Hertz prepared four diagrams to illustrate the nature of the field in the neighbourhood of an oscillator giving a stable wave train. His theory requires modification, owing to every Hertzian oscillator really giving a rapidly damped wave train. Miss Lee's diagrams illustrated the changes in the field during 6½ complete oscillations. Four systems of curves gave the points of the field with relative strengths 50, 30, 10 and 1. The decadence of the field was represented not only by the gradual change of shape of the curves, but by the complete disappearance of the curves of greater strength. When the series is complete, it is proposed to reduce it by photography and use it in a "wheel of life," to illustrate the decadence of an oscillator-field.

A curious model for showing the gyroscopic properties of a wheel was exhibited by Mr. Killingworth Hedges. The wheel was represented by a rim, having within it a heavy inner disc which could be made to revolve rapidly on the axis of the wheel. When the wheel was allowed to roll slowly down an inclined plane, and the inner disc was made to revolve in the same direction as the wheel, they both assisted to keep the system upright. When, however, the inner disc revolved in the opposite direction to the wheel, the system was in a state of unstable equilibrium which caused a rapid revolution through 180°, when both the wheel and the inner disc revolved in the same direction, and so produced a state of stable equilibrium.

Four photographic views taken by Mr. W. Bartier, and illustrating the accumulation of ice on the river near the Beckton Gas Works, North Woolwich, in February of this year, were shown by Mr. G. J. Symons.

Photographs of curvilinear crystals of water were exhibited by Dr. Gladstone, F.R.S. The photographs were taken during the severe frost of last February, and showed the forms assumed by the vapour when frozen upon a shop window, and the glass roof of a photographer's studio. All the lines of the crystals were curved. Another exhibit by Dr. Gladstone consisted of a blue photograph showing the way in which a solution of sodium salts mixed with some earthy matter and water may be made to crystallise on evaporation. This specimen showed many spiral forms. It, and the original specimens, were prepared by Mrs. M. Watts Hughes.

Prof. A. G. Greenhill and Mr. T. I. Dewar exhibited an algebraical spherical catenary. By a special choice of the constants, depending upon the quinquisection of the period of the associated elliptic functions, the general equations of the spherical catenary, considered by Clebsch in Crelle, 57 were shown reduced so as to make the projection of the chain on a horizontal plane a closed algebraical curve of the tenth degree.

A number of interesting Japanese pictures, selected to illustrate the effects of time on the pigments used by the old painters of Japan (A.D. 1322 to the early part of the 19th century), were exhibited by Mr. W. Gowland. The chief pigments used in these pictures were as follows:—Greens and blues: carbonates of copper. Permanent blue: the mineral *Lapis lazuli*. Reds: red oxide of iron, vermilion, carmine. Permanent white: levigated oyster-shells. Black: soot prepared from the oil of *Sesamum Indicum*.

Tropical American butterflies, selected to show the existence of common colour-types among species associated in the same areas, were exhibited by Mr. W. F. H. Blandford. The phenomenon (*Homœochromatism*) is observed chiefly among species of the sub-families *Danainæ* and *Heliconiinæ*, but frequently species of other sub-families conform to the prevailing colour-type. To a particular class of cases of colour-resemblance the term "mimicry" has been applied. The series shown comprised:—(1) Species of *Heliconius* associated in pairs, the colour-type varying with the distribution from north to south. (2) Species of different genera (*Tithorea* and *Heliconius*) associated in pairs, and sometimes mimicked by butterflies of other families. (3) Homœochromatic types from various districts represented by numerous species in different families, sub-families and genera.

Minutiæ in finger-prints formed the exhibit of Mr. Francis Galton. The exhibit furnished an illustration of the exceptional trustworthiness of the finger-print method in determining questions of identity. It demonstrated that in a case of twins, whose portraits, classificatory measures, and finger-print formulæ were closely alike, the finger-print minutiæ were quite different. A second exhibit of Mr. Galton's was the print of the hand of a child eighty-six days old. An enlargement of this print showed the development of the ridges at that early age.

Mr. B. Harrison exhibited Eolithic implements from the chalk plateau of Kent. The implements were found by the exhibitor in pits, dug under the auspices of the British Association. Stones were shown which were thought to bear evidence of use as tools, naturally of suitable shapes, but improved upon by chipping round the edges where required.

The Curator of the Maidstone Museum showed a series of nine photographs (with map, ground plan, and section) of a supposed Roman Mithræum or Mithraic temple discovered on the east bank of the river Medway at Wouldham, near Maidstone. The temple, or "cave," was found by workmen while engaged in removing sand for ballast, and excavated under the superintendence of the exhibitor. It had apparently been built into the bank, standing east and west, measuring 40 feet in length and 20 feet in width. Numerous fragments of tiles, samian and other pottery, animal bones, and a coin of Constantinopolis were found in the *filling*, but no statuary or inscriptions. So far this "cave" is the only one found south of the river Tyne.

Mr. G. F. Scott Elliot had on view photographs and objects illustrating his recent expedition to Ruwenzori. The photographs showed characteristic trees and shrubs of Taru, view of Kagera River, and of Ruwenzori. The objects consisted of Wandorobbo costume, sword, quiver, fire-stick, and arrows; Uganda pottery, bark cloths, &c.; banana meal, &c., in form, ready for export.

Mrs. Ellis Rowan exhibited Australian wild flowers in water-colours. The examples were from Northern Queensland and Western Australia.

A letter and original manuscript of Emin Pasha's last Ornithological Journal formed an interesting exhibit by Sir William H. Flower. The objects were found by the officers of the Congo Free State, after Emin had been murdered by the Arabs at Kinena, on or about October 28, 1892.

A series of cultures of various forms of the bacteria which had been isolated from the river Thames, and then cultivated by the methods employed in the laboratory, formed Prof. Marshall Ward's exhibit. The bacteria were grouped in sections corresponding to the different types, and characterised by differences as to the pigment-production, temperature of growth, capacity of forming spores, behaviour in different media, sizes, shapes, and power of movement, &c. Some of them belonged to common species; others were rare, or unknown, and not classified.

An instrument for describing parabolas by means of a combined sliding and link motion was exhibited by the inventor, Mr. H. Thomson Lyon.

Sir David Salomons showed new forms of "top" slides for the lantern, selenite and hot-water slide heated electrically; and illustrated the behaviour of a glow lamp in the magnetic field, &c.

Mr. F. Enock exhibited a living aquatic hymenopterous insect, *Polynema natans* (Lubbock), *Caraphractus cinctus* (Haliday), described by him in these columns a few weeks ago. This minute and most beautiful Hymenopteron was observed by Sir John Lubbock swimming or flying under water, crawling about weed, &c. The *Mymaridæ* (Hal.) all oviposit in the eggs of other insects; *Polynema natans*, according to Ganin, having been bred from eggs of a dragon fly, *Æschna*. The smallest of this family, *Camptoptera papaveris*, is but one eighty-fifth of an inch in length.

The bone structure in the dentary bone of *Gomphognathus*, a South African reptile, was illustrated by one of Prof. Seeley's exhibits. The bone structure in this fossil, which is of Permian age, is not distinguishable from the bone structure of a mammal, in the arrangement of the haversian canals and the lacunæ. Prof. Seeley also showed vertical sections through the maxillary and mandibular teeth from the same skull. This exhibit consisted of three vertical sections of the skull of *Gomphognathus* taken at the hinder termination of the hard palate, showing the conical forms of the single roots to the molar teeth, the flat transverse crowns to the teeth, and the way in which the mandibular teeth are opposed to those in the skull.

A sacred bone-trumpet, drum, and flute were exhibited by Dr. George Harley. The trumpet and tom-tom drum were from the temple of a Buddhist monastery in Thibet. They were made from the bones of priests—from their being supposed to be more religiously effectual. The trumpet when blown emits a rising and falling mournful wailing sound. The drum, when the knobs attached to its strings are rattled against the skins, gives a disagreeable harsh noise which is thought to drive the evil spirits out of the temple. The flute is a Carib Indian's, from Guiana, made from the tibia of a deer (*Coassus rufinus*). From it can be got the notes 1, 2, and 3, in the natural harmonic ratios of 6, 7, and 8, as in the French flageolet.

The following exhibits, with demonstrations by means of the electric lantern, took place in the meeting-room of the Society.

Lantern slides, illustrating the ethnography of British New Guinea, by Prof. A. C. Haddon. The slides illustrated the physical characters of different tribes inhabiting British New Guinea, some of the occupations of the people, several kinds of dances, and the distribution of dance-masks. A series of dwellings from one end of the Protectorate to the other was shown, and two types of canoes. Finally, illustrations of the decorative art of various districts were thrown upon the screen. Evidence was given in support of the view that British New Guinea is inhabited by true dark Papuans, and by two distinct lighter Melanesian peoples, one of whom may have come from the New Hebrides, and the other from the Solomon Islands.

Dr. J. Joly exhibited examples of colour photography, and described his method of obtaining them. The photographs were a realisation of composite heliochromy in a single image. The method of composite heliochromy requires three images superimposed by projection. In Dr. Joly's photographs the colour analysis and synthesis are carried out in the one image. The colours are the natural colours as they registered themselves upon the plate, and in no case altered after reproduction. The specimens shown were first attempts, produced with rough apparatus. The images showed a slightly grained appearance, but this is avoidable with proper appliances. The process of taking and reproducing the photographs differs in no way from ordinary photography upon the dry plate, save that the sensitive plate is exposed in the camera behind a screen lined in particular colours. The positive is subsequently viewed through a screen lined with three other colours; the three "fundamental colours," which upon the three-colour theory of vision are supposed to give rise to all our colour sensations.

ON THE TEMPERATURE VARIATION OF THE THERMAL CONDUCTIVITY OF ROCKS.[1]

§ 1. THE experiments described in this communication were undertaken for the purpose of finding temperature variation of thermal conductivity of some of the more important rocks of the earth's crust.

§ 2. The method which we adopted was to measure, by aid of thermoelectric junctions, the temperatures at different points of a flux line in a solid, kept unequally heated by sources (positive and negative) applied to its surface, and maintained uniform for a sufficiently long time to cause the temperature to be as nearly constant at every point as we could arrange for. The shape of the solid and the thermal sources were arranged to cause the flux lines to be, as nearly as possible, parallel straight lines; so that, according to Fourier's elementary theory and definition of thermal conductivity, we should have

$$\frac{k(\mathrm{M},\ \mathrm{B})}{k(\mathrm{T},\ \mathrm{M})}=\frac{[v(\mathrm{M})-v(\mathrm{T})]\div \mathrm{MT}}{[v(\mathrm{B})-v(\mathrm{M})]\div \mathrm{BM}},$$

where T, M, B denote three points in a stream line (respectively next to the top, at the middle, and next to the bottom in the slabs and columns which we used); $v(\mathrm{T})$, $v(\mathrm{M})$, $v(\mathrm{B})$ denote the steady temperatures at these points; and $k(\mathrm{T},\ \mathrm{M})$, $k(\mathrm{M},\ \mathrm{B})$, the mean conductivities between T and M, and between M and B respectively.

§ 3. The rock experimented on in each case consisted of two equal and similar rectangular pieces, pressed with similar faces together. In one of these faces three straight parallel grooves are cut, just deep enough to allow the thermoelectric wires and junctions to be embedded in them, and no wider than to admit the wires and junctions (see diagram, § 8 below). Thus, when the two pieces of rock are pressed together, and when heat is so applied that the flux lines are parallel to the faces of the two parts, we had the same result, so far as thermal conduction is concerned, as if we had taken a single slab of the same size as the two together, with long fine perforations to receive the electric junctions. The compound slab was placed with the perforations horizontal, and their plane vertical. Its lower side, when thus placed, was immersed under a bath of tin, kept melted by a lamp below it. Its upper side was flooded over with mercury in our later experiments (§§ 6, 7, 8), as in Hopkins' experiments on the thermal conductivity of rock. Heat was carried off from the mercury by a measured quantity of cold water poured upon it once a minute, allowed to remain till the end of a minute, and then drawn off and immediately replaced

[1] A paper by Lord Kelvin, P.R.S., and J. R. Erskine Murray, read the Royal Society on May 30.

by another equal quantity of cold water. The chief difficulty in respect to steadiness of temperature was the keeping of the gas lamp below the bath of melted tin uniform. If more experiments are to be made on the same plan, whether for rocks or metals, or other solids, it will, no doubt, be advisable to use an automatically regulated gas flame, keeping the temperature of the hot bath in which the lower face of the slab or column is immersed at as nearly constant a temperature as possible, and to arrange for a perfectly steady flow of cold water to carry away heat from the upper surface of the mercury resting on the upper side of the slab or column. It will also be advisable to avoid the complication of having the slab or column in two parts, when the material and the dimensions of the solid allow fine perforations to be bored through it, instead of the grooves which we found more readily made with the appliances available to us.

§ 4. Our first experiments were made on the slate slab, 25 cm. square and 5 cm. thick, in two halves, pressed together, each 25 cm. by 12·5, and 5 cm. thick. One of these parts cracked with a loud noise in an early experiment, with the lower face of the composite square resting on an iron plate heated by a powerful gas burner, and the upper face kept cool by ice in a metal vessel resting upon it. The experiment indicated, very decidedly, less conductivity in the hotter part below the middle than in the cooler part above the middle of the composite square slab. We supposed this might possibly be due to the crack, which we found to be horizontal and below the middle, and to be complete across the whole area of 12½ cm. by 5, across which the heat was conducted in that part of the composite slab, and to give rise to palpably imperfect fitting together of the solid above and below it. We therefore repeated the experiment with the composite slab turned upside down, so as to bring the crack in one half of it now to be above the middle, instead of below the middle, as at first. We still found, for the composite slab, less conductivity in the hot part below the middle than in the cool part above the middle. We inferred that, in respect to thermal conduction through slate across the natural cleavage planes, the thermal conductivity diminishes with increase of temperature.

§ 5. We next tried a composite square slab of sandstone of the same dimensions as the slate, and we found for it also decisive proof of diminution of thermal conductivity with increase of temperature. We were not troubled by any cracking of the sandstone, with its upper side kept cool by an ice-cold metal plate resting on it, and its lower side heated to probably as much as 300° or 400° C.

§ 6. After that we made a composite piece, of two small slate columns, each 3·5 cm. square and 6·2 cm. high, with natural cleavage planes vertical, pressed together with thermoelectric junctions as before; but with appliances (see § 10) for preventing loss or gain of heat across the vertical sides, which the smaller horizontal dimensions (7 cm., 3·5 cm.) might require, but which were manifestly unnecessary with the larger horizontal dimensions (25 cm., 25 cm.) of the slabs of slate and sandstone used in our former experiments. The thermal flux lines in the former experiments on slate were perpendicular to the natural cleavage planes, but now, with the thermal flux lines parallel to the cleavage planes, we still find the same result, smaller thermal conductivity at the higher temperatures. Numerical results will be stated in § 12 below.

§ 7. Our last experiments were made on a composite piece of Aberdeen granite, made up of two columns, each 6 cm. high and 7·6 cm. square, pressed together, with appliances similar to those described in § 6; and, as in all our previous experiments on slate and sandstone, we found less thermal conductivity at higher temperatures. The numerical results are given in § 12.

§ 8. The accompanying diagram (Fig. 1) represents the thermal appliances and thermoelectric arrangement of §§ 6, 7. The columns of slate or granite were placed on supports in a bath of melted tin with about 0·2 cm. of their lower ends immersed. The top of each column was kept cool by mercury, and water changed once a minute, as described in § 3 above, contained in a tank having the top of the stone column for its bottom, and completed by four vertical metal walls fitted into grooves in the stone, and made tight against wet mercury by marine glue.

§ 9. The temperatures $v(B)$, $v(M)$, $v(T)$ of B, M, T, the hot, intermediate, and cool points in the stone, were determined by equalising to them successively the temperature of the mercury thermometer placed in the oil-tank, by aid of thermoelectric circuits and a galvanometer used to test equality of temperature by nullity of current through its coil when placed in the proper circuit, all as shown in the diagram. The steadiness of temperature in the stone was tested by keeping the temperature of the thermometer constant, and observing the galvanometer reading for current when the junction in the oil-tank and one or other of the three junctions in the stone were placed in circuit. We also helped ourselves to attaining constancy of temperature in the stone by observing the current through the galvanometer, due to differences of temperature between any two of the three junctions B, M, T placed in circuit with it.

§ 10. We made many experiments to test what appliances might be necessary to secure against gain or loss of heat by the stone across its vertical faces, and found that *kieselguhr*, loosely packed round the columns and contained by a metal case surrounding them at a distance of 2 cm. or 3 cm., prevented any appreciable disturbance due to this cause. This allowed us to feel sure that the thermal flux lines through the stone were very approximately parallel straight lines on all sides of the central line BMT.

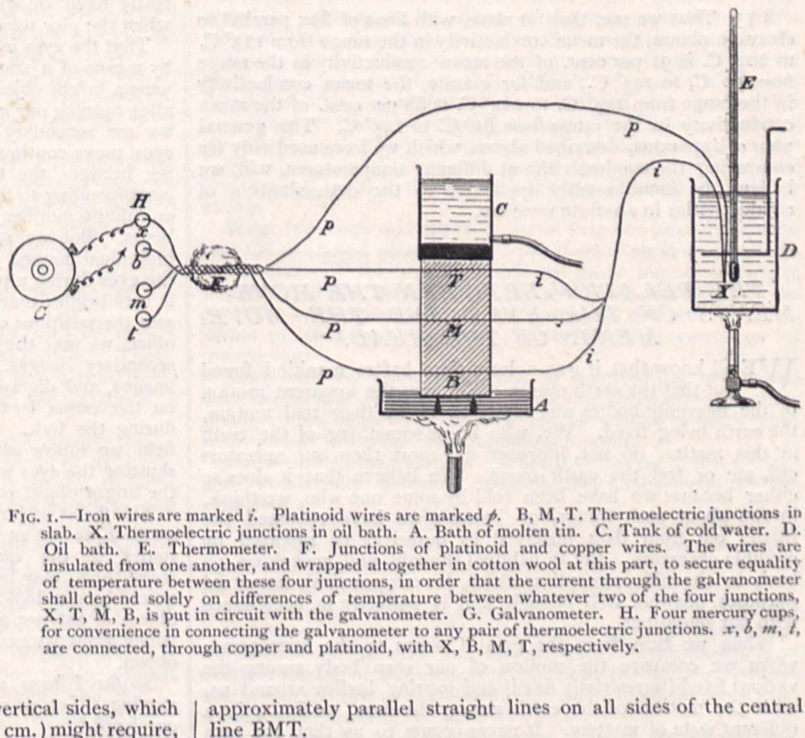


FIG. 1.—Iron wires are marked *i*. Platinoid wires are marked *p*. B, M, T. Thermoelectric junctions in slab. X. Thermoelectric junctions in oil bath. A. Bath of molten tin. C. Tank of cold water. D. Oil bath. E. Thermometer. F. Junctions of platinoid and copper wires. The wires are insulated from one another, and wrapped altogether in cotton wool at this part, to secure equality of temperature between these four junctions, in order that the current through the galvanometer shall depend solely on differences of temperature between whatever two of the four junctions, X, T, M, B, is put in circuit with the galvanometer. G. Galvanometer. H. Four mercury cups, for convenience in connecting the galvanometer to any pair of thermoelectric junctions. *x*, *b*, *m*, *t*, are connected, through copper and platinoid, with X, B, M, T, respectively.

§ 11. The thermometer which we used was one of Casella's (No. 64,168) with Kew certificate (No. 48,471) for temperature from 0° to 100°, and for equality in volume of the divisions above 100°. We standardised it by comparison with the constant volume air thermometer[1] of Dr. Bottomley with the following result. This is satisfactory as showing that when the zero error is corrected the greatest error of the mercury thermometer, which is at 211° C., is only 0·3°.

Reading. Air thermometer.	Reading. Mercury thermometer.	Correction to be subtracted from reading of mercury thermometer.
° 0	1·9	1·9
120·2	122·2	2·0
166·8	168·6	1·8
211·1	212·7	1·6
265·7	267·5	1·8

[1] *Phil. Mag.*, August 1888, and *Roy. Soc. Edin. Proc.*, January 6, 1888.

§ 12. Each experiment on the slate and granite columns lasted about two hours from the first application of heat and cold; and we generally found that after the first hour we could keep the temperatures of the three junctions very nearly constant. Choosing a time of best constancy in our experiments on each of the two substances, slate and granite, we found the following results:—

Slate: flux lines parallel to cleavage.

$$v(\mathrm{T}) = 50^\circ{\cdot}2\ \mathrm{C}.$$
$$v(\mathrm{M}) = 123^\circ{\cdot}3.$$
$$v(\mathrm{B}) = 202^\circ{\cdot}3.$$

The distances between the junctions were BM = 2·57 cm. and MT = 2·6 cm. Hence by the formula of § 2,

$$\frac{k(\mathrm{M, B})}{k(\mathrm{T, M})} = \frac{73{\cdot}1 \div 2{\cdot}6}{79{\cdot}0 \div 2{\cdot}57} = \frac{28{\cdot}1}{30{\cdot}7} = 0{\cdot}91.$$

Aberdeen granite:

$$v(\mathrm{T}) = 81^\circ{\cdot}1.$$
$$v(\mathrm{M}) = 145^\circ{\cdot}6.$$
$$v(\mathrm{B}) = 214^\circ{\cdot}6.$$

The distances between the junctions were BM = 1·9 cm. and MT = 2·0 cm.

$$\frac{k(\mathrm{MB})}{k(\mathrm{TM})} = \frac{64{\cdot}5 \div 2{\cdot}0}{69{\cdot}0 \div 1{\cdot}9} = \frac{32{\cdot}2}{36{\cdot}3} = 0{\cdot}88.$$

§ 13. Thus we see, that for slate, with lines of flux parallel to cleavage planes, the mean conductivity in the range from 123° C. to 202° C. is 91 per cent. of the mean conductivity in the range from 50° C. to 123° C., and for granite, the mean conductivity in the range from 145° C. to 214° C. is 88 per cent. of the mean conductivity in the range from 81° C. to 145° C. The general plan of apparatus, described above, which we have used only for comparing the conductivities at different temperatures, will, we believe, be found readily applicable to the determination of conductivities in absolute measure.

THE RELATION BETWEEN THE MOVEMENTS OF THE EYES AND THE MOVEMENTS OF THE HEAD.[1]

We all know that it was a long time before mankind found out that the earth moves. For ages the apparent motion of the heavenly bodies was supposed to be their real motion, the earth being fixed. We, who know something of the truth in this matter, do not, however, any more than our ancestors did, see or feel the earth move. We believe that it does so either because we have been told by some one who, we think, knows about such things, or because we have reasoned the matter out from data observed by ourselves or reported by credible observers. But in habitual thought and speech we go back to the old assumption which, for our practical, terrestrial purposes, answers well enough, and is perfectly in accordance with our sensations.

When we turn from the great Cosmos to the microcosm; when we compare the motion of our own body among the various fixed (terrestrially fixed) and moving bodies around us, with the motion of the earth among the stars, we find quite a different state of matters. It never occurs to us that our own body is at rest, and that the trees, houses, &c., move. When we really move we not only know, but feel and see that we are moving, and every one learned or ignorant, old or young—if only he is sober—feels and sees that the solid earth is fixed, except on the rare occasion of an earthquake, and in the case of some illusions which we shall have to consider. I wish to discuss the cause of this *sensation* of the fixedness of the earth, and also incidentally of the exception implied in the words I have just used, "if only he is sober."

If we keep our head fixed and look at any really fixed scene—say, a room in which there is nothing moving—or a landscape, if we can find one without railway trains, ships, moving beasts, or flying birds, we can allow our eyes to run over it in as uniform or as irregular a way as we please, and see that the scene remains fixed. We might have supposed that, as we move our eyes from right to left the whole scene, like a moving panorama, would seem to move from left to right, but it does not do so. It remains visibly at rest, and we know, without any reasoning about it, that the changes of view were produced by the motion of our eyes.

We fancy that we can move our eyes uniformly, that by a continuous motion like that of a telescope we can move our eyes along the sky-line in the landscape or the cornice of the room, but we are wrong in this. However determinedly we try to do so, what actually happens is, that our eyes move like the seconds hand of a watch, a jerk and a little pause, another jerk and so on; only our eyes are not so regular, the jerks are sometimes of greater, sometimes of less, angular amount, and the pauses vary in duration, although, unless we make an effort, they are always short. During the jerks we practically do not see at all, so that we have before us not a moving panorama, but a series of fixed pictures of the same fixed things, which succeed one another rapidly. It is not difficult to understand how this gives rise to a sensation of the fixedness of the external scene. If, in the otherwise fixed scene, there is a really moving object, we see it move, because during the pauses, short as they are, the moving object has visibly changed its place, and in each of our fixed pictures the moving object is seen to move. If it moves too slowly for this, then we do not *see* it move, but only infer its motion from comparison of its position at different times. If we keep our eyes fixed on the moving object, and this is possible if it does not move too fast or too irregularly, then we see it fixed and the really fixed things moving, an illusion we have all observed when the pier seems to move and the steamer remain at rest.

That the eyes jerk in the way now stated can be made plain by means of a simple experiment. If we have in the field of view a bright object, such as an incandescent electric lamp, and after running our eyes over the scene before us, shut our eyes, we see secondary images of the bright object.[1] Now if the eyes move continuously from one position to another, we should see between the two secondary images of the bright object corresponding to these two positions, a bright band composed of an infinite number of images each infinitely near its two neighbours. But we see no such band, but a finite number of sharp individual images, each of which corresponds to the position of the eyes during a pause between jerks; unless the bright object is very bright, there is nothing in the secondary image to represent the positions of the eyes during the jerk. If for a bright object we take the sun, then we do see bands joining the sharp secondary images. These bands are fainter than the sharp images, and die away sooner. They are the impressions made on the retina by the image of the sun passing rapidly across it during the jerk. But, if with the fixed bright object in the field we follow with our eyes a really moving thing, then on shutting the eyes we see a band of light, because the image of the bright object passed not very rapidly across the retina.

This habit of jerking the eyes from one position of vision to another, as fast as the light, well-poised globes can be swung round by the quick-working, straight-fibred muscles which move them, may be an innate habit, or it may have been acquired by our looking at things and turning quickly from one object of interest to another; at all events, it is now the way in which alone we can move them, unless we fix them on a moving object.

So far I have supposed the head fixed and the eyes alone moving. Let us now attend to what happens when we move our head.[2]

The movement of the head, unless it is very rapid, makes no difference at all in the phenomena just described.

If we call the line along which we look during the pause between the two jerks a glance-line, we may describe the whole phenomenon by saying that the glance-lines are fixed relatively to fixed external objects, whether the head is rotated or not. This, of course, means that, during a pause, the eyes are rotated relatively to the head about the axis about which the head is really rotated, in the opposite sense and through the same angle as the head.

It might, for all that has been yet said, be supposed that this fixedness of the glance-lines, when the head is rotated, depends on the habit of looking at things; but that this is not the cause, or, at all events, not the only cause, is plain from the fact that the same relative movements of the eyes take place when we

[1] Being the Fourth "Robert Boyle" Lecture, delivered before the Oxford University Junior Scientific Club, at the University Museum, Oxford, on May 13, 1895, by Prof. A. Crum Brown, F.R.S.

[1] The secondary images are better seen if we look at a white surface and wink rapidly.

[2] By "moving the head," I mean moving the head either alone or along with the body or any part of it.

look at an objectless field of view, such as the clear, cloudless sky, or, as was, I believe, first noticed by Dr. Breuer, when the eyes are shut. By placing the fingers lightly over the closed eyelids we can feel the motion of the prominent cornea. If, with eyes shut and fingers so placed on the eyelids, we turn the head or turn head and body round, we feel the eyes twitch. As the head turns round the eyes retain for a little a fixed orientation in respect to external fixed things, and then jerk so as to make up for lost time, again pause, and again jerk, and so on. So that while the head turns uniformly, the eyes, which must, of course, on the whole make one full turn, while the head makes one full turn, do their rotation intermittently, being, so to speak, left behind by the head, and then making up by a rapid jerk.

Another proof that these compensatory movements, as they may be called, of the eyeballs are not, or, at least, not wholly, caused by the effort of looking at things, is afforded by observing what happens when the head is rotated about a fore and aft axis, about an axis coinciding with a glance-line. If we keep our eyes fixed on a particular point and rotate the head about the line along which we look,[1] we still see things fixed, the world does not seem to revolve about our fore and aft axis. Here also we can show by means of secondary images that we see a series of fixed pictures.

If, with a bright object in the field of vision, we fix our eyes and keep them fixed on a point, about 15° distant from the bright object (if we keep both eyes open, about as far from our eyes as the bright object is, so as to avoid double vision), and then rotate the head about a fore and aft axis through, say, 30° by inclining the head towards one shoulder, and shut the eyes after this performance, we see a number of sharp secondary images of the bright object arranged upon an arc of a circle, the radius of which is the angular distance of the bright object from the point fixed.

If I have rotated my head through about 30°, I see about five secondary images, so that what I call the *angle of rotatory nystagmus* is, in my case, about 6°. Here we have been looking all the time at the same point, and it is not easy to suppose that the very slight attention we pay to objects seen indirectly, or, as we sometimes say, "with the tail of the eye," could lead to a habit, so fixed that we cannot escape it, of moving the eyeballs in the way described.

I have said that the movement of the head, *unless it is very rapid*, does not affect the fixedness of the glance-lines. Translatory motion of our body may be so rapid, as in a railway train, that the eyes cannot twitch so fast as to keep the glance-lines fixed relatively to near fixed objects.

The eyes do their best, they twitch but not enough, unless the train is moving slowly, and near objects seem to fly backwards. We succeed with fixed objects at a greater distance from us; we can see them fixed, and all objects between us and such visibly fixed objects are seen to move backwards, fixed things beyond them seem to move forward with us. Of course if, by keeping our attention on our carriage and its contents, our glance-lines become fixed in reference to these really moving things, they seem fixed, and the whole world outside of the carriage is seen to move in the direction opposite to that of our real motion. It is also obvious that rotation of the head, if it is more rapid than the quickest possible rotation of the eyeball in the head, must affect the position of a glance-line, for, in order that the glance-line may remain fixed, the eyeball must rotate in reference to the head as fast in one sense as the head rotates in reference to external things in the other sense; but in the case supposed, the eyeball cannot do so. We can try this experiment without having recourse to mechanical means of rotating our body and head, which, of course, we could do as fast as we please, and a great deal faster than would be either pleasant or safe. The most rapid rotation of our head which we can produce by the direct action of our muscles is what is known as wagging, that is, a rotation about a vertical axis upon the joint between the first two vertebræ. In this way we can give the head an angular velocity considerably greater than the maximum angular velocity of the eyeball. When we do this as fast as we can, we see that external things do not appear steady. When we wag our head to the right we see the world wag to the left, and *vice versa*. But the external really fixed things do not appear to us to describe nearly so large an angle as the head really does, the eyes make an effort to compensate the rotation of the head, an effort only partially successful, the angle through which external things seem to move being the difference between the actual angular rate of movement of the head, and the maximum possible angular rate of movement of the eyeball in its socket. This difference can best be observed and, indeed, can be approximately measured by observing a distant light on a dark night, while we wag the head. The point of light seems drawn out into a horizontal line of light, the apparent length of which is the angular difference in question. As we can wag our head much faster than we can nod it, the apparent length of the vertical line of light into which a bright point is drawn out when we look at it and nod as rapidly as we can, is much less than that of the horizontal line of light just spoken of; but I find that I can, by nodding, rotate my head about a right and left axis a little faster than I can rotate my eyes about the same axis, so that the luminous point does appear drawn out into a short vertical line.

Such violent movements of the head occur sometimes in our ordinary (not experimental) use of our eyes, but they are rare and isolated, so that the disturbance of the fixedness of the glance-lines which they cause does not really affect our sense of the fixedness of the world. The illusion of the moving pier and fixed steamer, which we have all also observed when there is a train alongside that in which we happen to be, and we see the moving train fixed and the fixed train moving, is corrected by looking at the shore or the railway station. For a moment these also seem to move, but our glance-lines almost instantly become fixed in reference to these things which we know are fixed, and it is then difficult to recall the illusion. Another similar case is that of the moon and the clouds. We sometimes see the moon moving and the clouds fixed, sometimes the clouds moving and the moon fixed, as our glance-lines are fixed relatively to the clouds or to the moon, and a little practice enables us to change from the one sensation to the other at will.

What has been said seems to show that our immediate sense that the earth and what we call fixed objects on it are fixed is a consequence of the way in which we move our eyes, and, in particular, of the way in which, by a suitable movement of the eyeballs, we involuntarily and unconsciously compensate movements of the head, voluntary or involuntary, conscious or unconscious.[1]

That such an immediate sense of the fixedness of external fixed things is of great use to us in moving about among them is plainly shown when we observe the trouble which a drunken man, who has lost this sense, has in guiding himself.

I now turn to the question. What is the cause of this prompt and wonderfully accurate compensatory movement of the eyeballs?

There are three sources from which we can obtain information leading to an answer. (1) Experiments on ourselves, (2) anatomical observations and measurements, and (3) observations of the effects of injuries to the labyrinth of the internal ear.

I shall consider these in their order.

By experiments on ourselves I mean the study of the effect on the motion of the eyes and on our sense of the fixedness of external things, of movements of our head (in this case, always along with the rest of our body) which we do not make, as a rule, for any other purpose.

I have already stated that if we shut our eyes, place our fingers on the eyelids, and turn round about a vertical axis, we feel with our fingers the jerking motion of the eyeballs. If instead of turning once round, we turn round several times, still better if we seat ourselves on a turning-table and get some one else to turn it and us round at a uniform rate, we find that the jerks become less and less frequent, and after two or three turns cease altogether. Another thing which we observe is, that although the turn-table is being turned round at a perfectly uniform rate, we feel the rotation becoming slower and slower, and when the jerks of the eyeballs have quite ceased we feel ourselves at rest, and have no sensation of rotation. Let us for convenience call the sense in which the rotation is still going on positive. This uniform positive rotation has become to us imperceptible (as long as we keep our head in the same position in respect to the vertical), and is what we may call a new zero of rotation. If the rate of rotation is now increased, we feel this increase as a positive rotation; if it is diminished, we feel the diminution as a negative

[1] If we take a sufficiently distant object as the thing to be looked at, we may neglect the want of coincidence of the two glance-lines belonging to the two eyes, and, moreover, all that is here described is seen as well, though not so conveniently with one eye shut.

[1] I need hardly repeat that, by movements of the head, I mean movements of the head whether accompanied or not by movements of the body.

rotation—a rotation the other way about. What we really perceive then is *acceleration* of rotation, using the word acceleration in its technical sense. If the turn-table is stopped, this is a negative acceleration, and what we feel is that we are being turned round in a negative sense, and at the same time we feel our eyeballs jerk. The sense of rotation and the jerking die away in this as in the former case.

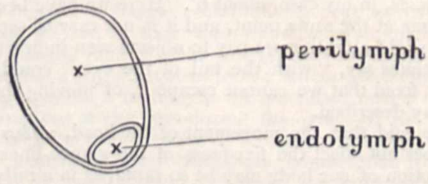
If, while we are being turned round with uniform angular velocity, but after all sense of rotation and all jerking of the eyeballs have ceased, we open our eyes, we still feel ourselves quite at rest, but we see all external objects turning round us; as has been well said by Prof. Mach, the external world seems to turn round inside an outer unseen fixed world. It is in reference to this imaginary fixed world that our glance-lines are now fixed. If the rate of rotation is changed while the eyes are open, the sensation of rotation is exactly the same as if they were shut, we feel the acceleration—positive or negative—as a rotation in the one or in the other sense, and the jerks of the eyeballs take place as if the real external world were not there, and we were looking beyond it at the unseen fixed world outside of it, that imaginary world in reference to which our glance-lines are now fixed.

If while the experiment I have described is going on, we move so as to change the direction, in our head, of the axis of rotation—for instance, if, after uniform rotation about a vertical axis has gone on, with the head in its usual upright position, until the sense of rotation has ceased, we bow our head forwards so that the axis of rotation is now parallel to a line from the occiput to the chin, a very striking, and somewhat alarming, but most instructive sensation is experienced. What we feel is that we are being turned round with a rotation which is the resultant of two rotations of equal angular velocity—one the real rotation about what is now the vertical, the other the imaginary (but equally perceived) rotation in the opposite sense about the line in the head which was vertical. If the angular movement of the head is small, so that the angle between what is the vertical and what was the vertical is small, then the two component rotations nearly neutralise one another, and the strange and alarming resultant is slight; but if the head is bent so that the old and new verticals are at right angles to one another, the real and the imaginary components are both felt in full, and the effect is very startling. If the rate of rotation is changed simultaneously with the change of position of the head, we have a resultant of two rotations of different angular velocity. The most easily observed case of this kind is when the rotation is stopped altogether at the moment of change of position of the head. Here the real component is zero, and we have only the imaginary one. This is the case of the well-known practical joke: a man is asked to plant the poker before him on the floor, place his forehead on the end of it, walk round it three times, and then rise and walk to the door. The preliminary part of this experiment presents no difficulty; the victim plants the poker, puts his forehead on it, walks round it with the greatest ease and with no sense of anything unusual. But when he rises, the line in his head which was vertical is now horizontal, and he feels himself turned round about that horizontal line. The external world he also sees turning round this line, objects on the one side rising up and objects on the other side sinking down. In this visibly swaying world he has to guide his sensibly rotating body, and if his friends do not catch hold of him he is pretty sure to fall. All these experiments are most conveniently made on a smoothly working turn-table of such a size that one can comfortably lie down upon it. By the kindness of Messrs Dove, lighthouse engineers, I had the use of a large turn-table made for the revolving lantern of a lighthouse. It could be turned round smoothly and uniformly, at the moderate speed that is most suitable for experiments of the kind in question. A few experiments with such an apparatus will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body.

This is one of the ways in which the phenomena have been explained by those who hesitate to believe that there can be a definite special sense only discovered within the last few years. That the origin of the sensation is not in the soft parts of the body generally, but in the head, is made perfectly plain by the fact that the position of the head and the changes of that position alone determine the sensations. We must therefore look in the head for the organ of this sense.

In close proximity to the cochlea, which is universally regarded as the organ of hearing, there is an organ of very striking, and we might say mysterious, form. It occurs in all vertebrates, and occurs in them fully developed, except in the lowest forms of fish. It is contained in a bony or cartilaginous cavity, which communicates with the cochlea or lagena. This cavity may be divided into the vestibule and the three semicircular canals. The canals open at both ends into the vestibule, and each has at one end an enlargement called the ampulla.[1] Within this bony case is contained a membranous structure, consisting of the utricle, situated in the vestibule, and three membranous canals, each in one of the bony canals, each with an ampulla in the bony ampulla, and each opening at both ends into the utricle. The vestibule contains, besides the utricle, the saccule, a membranous bag continuous with the cochlear duct, and has in the side next the tympanic cavity a hole in the bony wall filled in by a membrane, and known as the *fenestra ovalis*. The saccule and the utricle have each a spot on the lower wall supplied with nerves ending in hair-cells, and known as the *macula acustica*.

The *maculæ acusticæ* are probably, as suggested by Mach and Breuer, organs fitted to perceive acceleration of translatory motion, and are not connected directly with the function of the semicircular canals. The *fenestra ovalis* belongs to the organ of hearing, which may thus be said to have a right of way through the vestibule. We need not therefore here consider any further these organs, but confine ourselves to the semicircular canals and the utricle in its relation to them. As already stated, each bony canal contains a membranous canal. The membranous canal is, except at the ampulla, much smaller in bore than the bony canal, so that the space outside the membranous canal filled with perilymph, is much greater than the space inside filled with endolymph. The membranous ampulla much more nearly fills the bony ampulla, so that here the perilymph space is comparatively small. The membranous canal is pretty firmly attached (in some animals, at all events) to the periosteum of the bony canal, so that in man a section has somewhat this form:



Each canal is, in all animals I have examined, approximately in a plane, and it is important to consider the relations of these planes to one another and to the mesial plane of the head.

As I have brought part of the apparatus with me, I may shortly describe the method I used to measure the angles which these planes make with one another, and also an improved method, of which I have not yet had time to make any very full trial.

[The method illustrated by the human skull shown is fully described, with woodcuts from photographs, in Prof. McKendrick's "Text-book of Physiology," vol. ii. pp. 697-699, and therefore need not be reprinted here. The other method will, I hope, give more accurate measurements.]

It consists in attaching the preparation—either a cast of the canals, or, in the case of a bird, the dissected and cleaned bony canals—to one arm of a branched rod, and a lump of wax to the other. The rod is then fixed to the large apparatus already referred to. The canals are successively made horizontal, and a small plate of glass fixed horizontally in each case—parallel therefore to each canal—to the lump of wax. We can also attach a glass plate parallel to the mesial plane. We can thus have, on a comparatively small piece of wax, glass plates parallel to all the planes, the relations of which to one another are to be measured. The lump of wax is then removed from the rod, and the angles between the planes of the glass plates measured by means of an ordinary reflexion goniometer.

The general results are:

(1) The canals do not lie rigorously in planes, but sufficiently nearly so to give closely accordant results.

(2) The external canals are very nearly at right angles to the mesial plane, and therefore, from the bilateral symmetry, the two external canals are very nearly in one plane.

(3) The superior and posterior canals of the same side make

[1] In all animals the non-ampullary ends of the superior and the posterior canal have a common opening into the vestibule.

approximately equal angles with the mesial plane. In all cases which I have examined, the angle between the posterior canal and the mesial plane is somewhat larger than that between the superior canal and the mesial plane.

From the bilateral symmetry, therefore, the superior canal of the one side is nearly, but not quite, parallel to the posterior canal of the other side. In the discussion of the way in which the system of canals may be supposed to act, I shall for convenience assume that these canals are parallel, as the deviation from exact parallelism only complicates, but does not at all vitiate, the argument.

(4) In man, and in a large number of other animals, the three canals are very nearly at right angles to one another. But, in a good many of the animals I have looked at, the superior and posterior canals make with one another an angle considerably greater than a right angle.

Looking at the six canals as forming one system, we see that we have three axes, that at right angles to each axis there are two canals, one in the one internal ear, the other in the other; these two canals having their ampullæ at opposite ends, so that if rotation takes place about the axis, the ampulla in the one case precedes the canal, in the other follows it. The vertical axis, as we may call that at right angles to the two external (or horizontal) canals, is pretty nearly vertical in most animals, in the usual position of the head when the animal looks to the horizon; in man it is not exactly so, we must bow our head a little to make this axis vertical. If we suppose we are looking north, the other two axes are north-east and south-west and north-west and south-east respectively. In man they pass from the eye of one side to the mastoid process of the other side, and are nearly at right angles to one another. As already stated, in some animals they are inclined and are nearer the right and left than the fore and aft line in the head.

In order to see how such a system can work as a hydrodynamical instrument, let us first consider one canal.

Here we have two watery liquids, the endolymph within the membranous canal, its ampulla and the utricle, the perilymph between these and the bony case. How will these behave when rotation takes place about an axis normal to the plane of the canal? The inertia of the liquids will tend to produce a flow through the canal in the sense opposite to that of the rotation.

Let the rotation take place so that the ampulla precedes the canal. Here the endolymph will tend to flow from the utricle into the ampulla, and thence through the canal to the utricle again. But, as Mach has pointed out, the canal has too small a bore to allow of any sensible flow through it, so that the effect of this rotation will be to increase the pressure within the membranous ampulla. But (and this is a point to which, as far as I know, no one has hitherto called attention) as there will also be a tendency of the perilymph to circulate, so in its circle there is also a narrow place, namely at the ampulla; for as the membranous ampulla nearly fills its bony case, there is not much room there for the perilymph to pass from the vestibule into the space surrounding the membranous canal. There will, therefore, be a diminution of pressure of perilymph at the ampullary end of the canal, so that the ampullary walls will be stretched by the increase of pressure within and the diminution of pressure without. Of course when the rotation is kept up uniformly for some time the pressure inside and outside of the membranous ampulla is soon equalised, and the stretching or relaxation ceases. With the cessation of the stretching the sensation must also cease.

If now the rotation is stopped the perilymph and endolymph will tend to move on, and pressure will be produced inside the membranous ampulla of that canal, which during the rotation moved with ampulla following the canal.

All this will of course be reversed when the rotation takes place with the ampulla following the canal; the pressure inside the membranous ampulla will be diminished, that without increased, and the walls will become flaccid.

In each membranous ampulla there is a so-called *crista acustica* where nerves terminate in hair-cells, and it is not difficult to suppose that stretching of the ampullary walls will irritate these nerve-endings, while a relaxation of the ampullary walls will produce no irritation. If this be so, then we have three axes each with an organ sensitive to rotation about it in either sense, and capable of discriminating between the two; and as every rotation of the head can be resolved into component rotations about these three axes, we have the means of perceiving the axis and what we may call the intensity of the rotation, or perhaps more correctly the rotational acceleration.

This hydrokinetic theory of the function of the semicircular canals was propounded at very nearly the same time by Prof. Mach of Prague, Dr. Breuer of Vienna, and myself. I give the names in the order of publication. The views expressed by us were not exactly the same, and the statement of the theory I have just given is any one of them with additions and corrections from the other two.

I have not thought it necessary to refer to the hydrostatic theory of Goltz, or, indeed, to give any details of the literature of the subject. A very full and accurate digest of almost every thing that has been written on the functions of the several parts of the labyrinth of the ear has been published in Russian by Dr. Stanislaus von Stein, and translated into German by Dr. C. von Krzywicki.

The theory as I have just described it might perhaps have been developed, as I have here developed it, from a consideration of the structure and position of the canals. But, as a matter of fact, this was not the historical order. It was the experiments of Flourens that first directed attention to these organs as having something to do with the equilibrium of the body.

In reference to these experiments and those made since by many able physiologists and skilled operators, I shall only say that the results seem to me to be consistent with the hydrokinetic theory. Certain of de Cyon's experiments, in which he increased the pressure in the canals by inserting in them small tangle plugs without producing any nystagmus or rotatory movements of the head, appear to contradict the theory. But increase of pressure in the bony canal can have no tendency to stretch the walls of the membranous ampulla, and therefore could not be expected, if the theory as I have stated it is correct, to produce a sensation of rotation; what is required, is that the pressure inside the membranous ampulla should be greater than that outside of it.

The symptoms observed in cases of disease of the internal ear also appear to support this hydrokinetic theory.

But the position of the canals in close anatomical relation to the organ of hearing had impressed on the minds of physiologists so obstinate an opinion that they must be connected with the perception of sound in some way or other, that even now many will not admit that they are the peripheral organs of a sense of rotation.

A favourite theory was (and there are still some who hold it) that the semicircular canals give us information as to the direction in which sound comes to us. There are two ways in which we can show that this view is erroneous.

(1) By considering the physical conditions.

The shortest sound wave which we can hear is so long compared with the dimension of the ear, that every part of the ear must be at any instant in the same phase of the wave. We must assume that, as far as the effect of such sound waves is concerned, the liquid contents of the internal ear are incompressible. It is as absurd to speak of sound-waves travelling round one of the canals as to say that it is high water at one end of a dock and low water at the other, at the same time.

(2) By experiments on the way in which we really do perceive the direction of sound. I shall describe two such experiments. (*a*) Let the observer close his eyes—for security it is best to bandage them—seat himself in a chair, and keep his head steady. Now let an assistant produce a sharp short sound. In showing this experiment to Section D of the British Association, at its meeting at Belfast in 1874, I used three coins in the way I show you now. The observer can tell with really astonishing accuracy whether the sound comes from the right or from the left, because he hears it louder in the nearer ear, but he is without any knowledge at all as to whether it comes from above or below, from the front or the back. He forms a judgment indeed on this point, but his judgment is usually wrong, often very ludicrously so.

The experiment is most striking when the click is produced in the mesial plane of his head, in which case he has not the binaural effect to help him. In this connection I may say that I know no experiment which illustrates so well the marvellous delicacy of our sense of relative loudness of sound, a very small deviation from the mesial plane being quite certainly recognised.

We have then with one ear no means of ascertaining the direction of sound if we keep the head fixed. How then do we ascertain the direction of sound? for we all know that we can do so with very considerable accuracy. This leads me to the second experiment. (*b*) Let the observer, still with eyes closed and bandaged, stand up and be at liberty to move his head. Let the assistant produce the clicking sound, not once only, but again and again at short intervals, always in the same place. The observer turns round until he faces the source of sound. He knows that he is facing it when he hears it equally loud in both ears, and hears it to the right when he turns a little to the left, and to the left when he turns a little to the right, that is the criterion of whether the source is behind or before him. Having now got the azimuth, he seeks the altitude. Moving his head about a right and left axis, he seeks the position in which he hears the sound best. He is now looking towards the source of the sound.

The concha of the external ear acts as a screen, and it is remarkable how much difference there is in the quality as well as in the loudness of most sounds with different altitudes.

Stand in front of a pipe from which water is rushing, and move the head round a right and left axis, bow, in fact, to the pipe, and a striking difference in the quality and loudness of the sound will be observed in the different positions of the head.

It may be said birds have no concha, and yet they perceive as well as we do the direction of sound. But there is a method by which, without any use of the action of the concha, and by purely binaural observations, we can ascertain the direction of sound. By one observation, as already described, we can find a plane containing the line along which the sound reaches us. That plane is at right angles to the line joining our two ears. By moving the head we can shift the line joining our two ears, and then by another similar observation obtain the plane at right angles to the new position of the line joining the two ears and containing the direction of sound. The direction of sound is the intersection of these two planes.

I do not think *we* use this method (although I have tried it and found it work), but we often see birds incline their heads when listening in such a way as to suggest that they use it.

There is another objection which is often brought against the theory I have been explaining. It is said, "Is it conceivable that there should be a special sense, common to man and all vertebrate animals, which has remained unknown till about twenty-two years ago? This is a sense invented, not discovered by scientific men, otherwise we should all have known about its existence at least."

This objection is not one to be met by contempt; it has a real basis, and as I believe this sense to be a real one, I feel bound to look for the cause of the incredulity.

A special sense is popularly understood to be a gateway of knowledge. Information as to external things comes to us in various ways, and each of these ways has from ancient time been recognised and named as a special sense. But this is not exactly the physiological way of looking at things. I may illustrate the difference by a sort of analogy. In a large business establishment the manager sits in his room upstairs. He has various ways of getting information. The post brings him letters, he looks at them; some he carefully considers and answers, others he looks at and puts into the waste-paper basket—but he has looked at them all. So we *see* things; many of the things we consider, take note of, others we pay no attention to—do not an hour later remember anything about them. But there are many messages which come to the business establishment and never reach the manager's room at all. They are attended to by clerks in the office. They are not futile, they are real messages and serve their purpose, a purpose essential to the carrying on of the business. If these were not attended to downstairs, the manager would very soon hear of it. So with us. There are what we may call sensory impressions which do not make their way to the conscious *Ego*, but are all the same properly attended to by what in us corresponds to the clerks. If our clerks neglect their work, the conscious *Ego* very soon becomes aware that there is something wrong.

In the case of the sense of rotation, ordinarily we pay no attention to its messages—the clerks at the sensory centres of the ampullary nerves, and at the motor centres of the muscles of the eyeballs, do all that is necessary. We perceive the result of their work in our visual sense of the fixedness of the outside world, and we do not trouble ourselves as to how the office work has been done.

But—and here I come to a matter I referred to early in this lecture—the office work is sometimes not well done, and the visual sense of the fixedness of the outside world is lost. If this is due to disease, we send for the doctor and ask him to find out what is wrong in the office, and, if he can, put it right. But there is a far more common cause of the loss of the visual sense of the fixedness of the outside world, one which it has not been left for two or three scientific men to discover in the last quarter of the nineteenth century. The most characteristic effect of alcohol is to make all reflex actions sluggish. Under the influence of a moderate dose of alcohol, what I have called the office work, goes on all right but not quite so fast as with no alcohol. The message arrives, and the answer is sent, but not quite so promptly. The conscious *Ego* may not note anything wrong, but a quantity of alcohol, far short of a dangerously poisonous dose, may delay the transmission of the signal to the muscles of the eyeball so much as to affect quite perceptibly the compensation of the movements of the head. A perfectly sober man sees the world wag a little when he wags his head very vigorously—a point of light is perceptibly drawn out into a horizontal line of light—the office work fails a little under such extreme pressure. But a little alcohol makes the office work fail more readily, and as the dose is increased it fails altogether, and the sense of the fixedness of the world is wholly lost. Even in such an extreme case of intoxication, short of paralysis, the drunken man may see the world steady, if only he can keep himself steady: I dare say we have all seen very drunken men walking quite straight, but with a preternatural fixedness of the head. If anything makes them move their head, they totter and reel. They move the head a little; that happens to them in consequence of a small and slow rotation of the head, which happens to us when we wag our head violently, and they reel and stagger just as we should reel and stagger if we tried to walk, violently wagging our head all the time.

Just as there are blind men and deaf men, so there are men who have lost or never had the sense of rotation. Such persons are almost always deaf-mutes. The close anatomical relation of the organ of hearing and the organ of the sense of rotation has this effect, that imperfect development of pathological injury of the one is usually associated with similar defect in the other. And experiments on deaf-mutes have shown that a large proportion of them are defective in the sense of rotation. This is shown by the absence of the normal jerking of the eyeballs when they are rotated, and by a perceptible insecurity in their gait. They do not reel as drunken men do, just as blind men find their way about much better than we could do if our eyes were bandaged up; they have learned to get on fairly well with the help of experience and their other senses.

I am not sure whether in this account of the sense of rotation, of its organ, and of the use of it, I have carried all my hearers with me, and convinced you of the real existence and real practical use of this sense. I hope, however, I have made it clear that the subject is worthy of attention, and that we have here matter for the careful consideration of physicists, physiologists, and psychologists.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—In a Convocation, held on Tuesday last, the University, or at least a section of it, displayed itself in an unfavourable light. The Convocation House was crowded, not because of the Statute on Research Degrees, which came before the House, and passed its final stage without opposition, but because of the proposal, which seemed to be a modest one, that Anthropology should be included among the subjects of the Final School of Natural Science, not as an extra, but as an equivalent subject. This proposal was from the first strongly opposed by a few members of Congregation, but passed the two readings in that body by substantial majorities. The opponents of the subject, however, were not content to accept the results of these votes, and issued an urgent whip to members of Convocation, with the result that the statute was rejected by 68 votes against 60. Presumably the philosophers, historians, and divines who succeeded in throwing out the statute at its final stage are pleased with their performance. To the outside world, which is less than ever convinced that education is comprised within the limits of the subjects of the School of Literæ Humaniores, their action will be but another instance of the incompetency of a section of the classical world to understand what is going on around them. The circular which was issued by the opponents of the statute was so artfully worded as to rouse theological suspicions. Reference was made to the undesirability of the comparative study of religions, and it was obvious that a considerable proportion of those who attended to vote against the measure, had come in obedience to a summons to resist the enemy, and were in no way qualified to form a judgment on educational matters. The larger proportion, however, consisted of those classical teachers whose belief it is that science may safely be ignored in a nineteenth century education, and that a "good general education" means only a training in the Greek and Latin languages, with a smattering of ancient history and philosophy. The result of the vote was a great disappointment to those who had hoped that the work of Prof. Tylor, Prof. Arthur Thomson, and Mr. H. Balfour, would find its fruition in a small but earnest school of anthropologists in Oxford.

THE National Association for the Promotion of Technical and Secondary Education has made arrangements for a Conference of the representatives of Technical Education Committees to be held at the Royal United Service Institution, on July 11, when the Duke of Devonshire, President of the Association, will take the chair. The object of this Conference is to discuss means whereby the various authorities charged with the provision of technical education may be brought into closer relationship, and may be enabled to avail themselves of the results of the experience of others as regards many important details of their work. Among the subjects which it is proposed to deal with are (*a*) scholarships (local conditions and uniformity in respect to award and tenure), (*b*) evening continuation schools (the co-ordination of their work with that of evening science, art, and technical classes), (*c*) trade and technology classes and their relation to the various trades.

THE chemical and engineering societies formed by the members of many of our polytechnic institutes might emulate, with advantage, the Engineering Society of the School of Practical Science, Toronto. We have lately received a volume of 253 pages containing the papers read before the Society during the session 1894-95. The papers refer to both the theoretical and practical sides of engineering, and their publication cannot but encourage investigation among the students. A plan adopted by this Society, and by a number of American societies of a similar kind, is worth noting. Before a paper is read, 150 proofs of it are distributed among engineers and specialists interested in the subject with which it deals, and their opinions upon any particular point are invited. The replies received are read after the paper, and help to make the discussion more general and of greater value than it otherwise would be.

THE Corporation of the Massachusetts Institute of Technology, Boston, have a good understanding of what technical education means. The following paragraph, from the *Calendar* of the Institute received a few days ago, should be borne in mind by the organisers of technical education in this country:—"The foundation of all sound technological education requires not only thorough theoretical training, but also prolonged, well-directed laboratory drill which shall first give the student the power of close and accurate observation, and then bring him into direct contact with the material problems of his future profession." It is by acting upon this educational principle that the Massachusetts Institute has gained such a large measure of success.

TABLES showing the number and proportion of pupils attending secondary schools in London are given in the *Technical Education Gazette*. The returns obtained show that the number of pupils receiving education in 84 public endowed and public proprietary schools is 19,072, and the number receiving education ni 126 private or semi-private schools is 7107. The proportion which pupils attending secondary schools bear to those attending public elementary schools, may be gathered from the fact that the number per 100,000 of the population attending secondary schools is 623, while the number per 100,000 of the population attending public elementary schools is 16,904.

SCIENTIFIC SERIALS.

Bulletin of the American Mathematical Society, vol. i. No. 8 (May 1895).—Kinetic stability of central orbits, by Prof. Woolsey Johnson, contains an investigation, of an elementary character, of a problem not discussed in the fourth edition (p. 125) of Tait and Steel's "Dynamics of a Particle." It is a satisfactory discussion of the problem so far as it relates to central orbits. The note was read before the Society at its April meeting.—Dr. J. Pierpont, in a short paper, read before the Yale Mathematical Club, entitled "Lagrange's place in the Theory of Substitution," though he cannot vindicate Lagrange's right to the title of creator of the theory of substitutions, presents a few examples of his methods in order to show the importance of considering him from this point of view. "Lagrange was led to the study of this theory by his attempts to solve equations of degree higher than the fourth."—Gauss's third proof of the fundamental theorem of algebra, by Prof. Bôcher, indicates the connection between Gauss's third proof that every algebraic equation has a root and those branches of mathematics which have since been developed under the names of the theory of functions and the theory of the potential. The notes, among other details, give the different courses of lectures in mathematics at American and European colleges.—There is the usual long list of new publications.

Wiedemann's Annalen der Physik und Chemie, No. 5.—Wave-lengths of ultra-violet aluminium lines, by C. Runge. The lines of the spark spectrum near 186 μ wave-length are of great intensity, and may be used as standards of reference. They were therefore carefully determined by means of a Rowland concave grating and sensitive plates prepared by Schumann's method. They were compared with the spectrum of iron, and referred to Rowland's standard wave-lengths for that substance. The figures for the four lines at 760 mm. pressure and 20° C. were 1854·09, 1862·20, 1935·29, and 1989·90. The wave-lengths reduced to a vacuum would be about 0·6 units greater.—On the dichroism of calcspar, quartz, and tourmaline for infra-red rays, by Ernest Merritt. The absorption of the infra-red rays in these substances depends upon the plane of polarisation. Especially in calcspar and in tourmaline the two curves representing the transmittency for the ordinary and the extraordinary ray, respectively, are quite different, so that they appear to be independent of each other. The following absorption bands were observed in these curves: Calcspar, at 2·44 μ and 2·74 μ for the ordinary ray. These are very sharp. Some broad bands also appear at 3·4 μ, 4 μ, and 4·6 μ. The extraordinary ray is absorbed at wave-lengths of 3·28, 3·75, and 4·66 μ. Quartz shows an absorption band for the ordinary ray at 2·9 μ. When the wave-length exceeds 4·75 μ the substance is practically opaque for both rays. Tourmaline absorbs the ordinary ray of wave-length 2·82 μ. The two curves intersect at 2·30 μ and again at 3·84 μ, so that between these two points the dichroism of tourmaline is reversed.—On the transmittency of solid bodies for the luminiferous ether, by L. Zehnder (see p. 153).—On the measurement of high temperatures with the thermo-element and the melting-points of some inorganic salts, by John McCrae. The melting-points of a number of salts, chiefly alkaline haloids, were determined by means of a platinum and platinum-rhodium couple, whose E.M.F. is proportional to the temperature between 300° and 1400°. The temperature of the alcohol flame, as shown by the same couple, was 1488°, and that of the Bunsen flame at its hottest part, 1725° C.—On electric resonance, by V. Bjerknes. This is an important contribution to the theory of Hertzian oscillations. The author considers the effect of the periods of the oscillator and the resonator, and their logarithmic decrements, together with a constant measuring the intensity of the oscillations. He thus arrives at several fundamental laws, such as: The secondary spark potential is proportional to the square of the period of the resonator, the magnetic or thermal integral effect to its cube, and the electric integral effect to its fifth power.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, June 14.—Captain W. de W. Abney, President, in the chair.—Mr. Burstall continued the reading of his paper on the measurement of a syclically varying temperature. Three sizes of platinum wire have been employed for the thermometers in order that some idea might be formed as to the magnitude of the error caused by the lag of the temperature of the wire behind that of the gases. The constants of the platinum thermometers were determined either by comparison with a standard Callendar platinum thermometer or by means of ice, boiling water and boiling sulphur. In most cases the thermometer constants were determined after the wire had been exposed to the action of the hot gases for about half an hour. One wire, however was calibrated before being used, and an unusually high value was obtained for the coefficient δ. After this wire had been exposed to the hot gases, the value of δ fell, however, to the normal. The author thinks the abnormal value may have been due to the formation of a gold platinum alloy during the process of attaching the wire to the leads, and that this alloy was subsequently swept off by the hot gases. The paper includes a number of tables and curves which embody the numerical results, and show that concordant results can be obtained on different days and with different thermometers. Prof. Perry said that an instrument for quickly recording varying temperatures was greatly required by engineers. The temperature just inside the cylinder walls was, however, the most important to determine, and a knowledge of how the temperature from 1 to 2 m.m. inside the walls varied would be of the greatest importance. He would like to ask the author if the observed temperatures agreed with the values calculated on the assumption that the gases in the cylinder behaved as a perfect gas, and that $\frac{PV}{T}$ was constant during the whole stroke. Differences between the observed and calculated values might be due to dissociation, and not entirely to lag in the thermometers. It was astonishing that even the fine wires employed were able to follow the rapidly varying temperature, and he would like to see some special experiments made to test this point. Prof. Capper showed a diagram giving the values calculated on the assumption that $\frac{PV}{T}$ = constant. In such a calculation it was necessary to assume some temperature as a starting-point, and in general this temperature was obtained from an analysis of the exhaust gases, so that the calculated curve is most likely to depart from the truth at the commencement of the stroke. He, Prof. Capper, hoped that the author would be able to accurately determine the temperature of some one point of the stroke, and he suggested that the point where the observed curve crossed the theoretical curve would be the most suitable one for this purpose. Such a point must exist, since at the commencement of the stroke the lag causes the observed temperature to be too low, while at the end of the stroke the observed temperatures are too high. Mr. Burstall finds a curious bump in his curves, and it is curious that a similar bump exists in the calculated curves. From the constancy with which this bump appears, it would seem that it must have some physical meaning. It was important to remember that the expansion in the gas-engine cylinder is not adiabatic, for heat is both abstracted and generated during the stroke. Mr. Blakesley suggested that since the temperatures dealt with were sufficient to make the wire red-hot, the question arose whether lag might be investigated by the wire being examined by means of Becquerel's phosphoroscope, at a known interval after the removal of the source of heat. Mr. Griffiths said he considered an important source of error was the large thermal capacity of the leads when the working wire was so very short. He thought it would be possible to standardise the thermometers under conditions similar to those which occur in the engine cylinder. Thus perhaps alternate gushes of air at 0° and 100° C. might be used. The use of gold to attach the fine wire to the leads was objectionable, since the gold must permeate the platinum for quite an appreciable fraction of the whole length of the wire. He would like to know whether the change in δ alluded to by the author occurred with the first explosion, or whether it was a gradual one. Mr. Enright pointed out that the nature of the working substance in a gas engine varied during the stroke. Prof. Perry said that the change in the specific volume of the gases before and after combustion did not amount to more than 1·25 per cent. Mr. E. Wilson said he thought it was most important to shorten the time of contact, since at present the galvanometer readings corresponded to the mean temperature over a range of about 5 per cent. of the whole stroke. It might be possible to make use of a condenser to get over this difficulty. Prof. Rücker said that the Kew Observatory were making arrangements to undertake the testing of platinum thermometers. Mr. Enright suggested that with a very short contact induced currents might cause errors. Mr. Rhodes said that he had found that the method of determining the zero point of thermometers, by means of melting ice, was far from satisfactory, and that the results obtained could not be depended upon to within 0°·1 C. The author, in his reply, said the only chemical action on the wires he thought probable was the formation of a carbide. After several hours use, however, the wires appear quite bright and clean.—Mr. N. F. Deerr read a paper on the thermal constants of the elements. The object of the paper is to establish the following laws: If T denote the melting-point on the absolute scale, C the mean coefficient of expansion between zero and the melting-point, S the mean specific heat, and L the latent heat of fusion, then, for any family in Mendeléef's periodic classification, the following relations will hold between metals and metals, and between non-metals and non-metals:

$$\left(T + \frac{L}{S}\right) C = \text{const.}$$
$$TC = \text{const.}$$
$$\frac{LC}{S} = \text{const.}$$

In the absence of other data, the mean values of C and S between 0° and 100° have been taken. Anomalous values are obtained in the case of gold and mercury, if these metals are included in their usual positions. The author considers that the thermal constants indicate that gold ought to be placed among the transition elements. He further proposes to place mercury in a new group to come before the lithium group. Such a group, he suggests, would contain hydrogen, argon and mercury. The paper concludes with an attempt to justify the expression

$$\left(T + \frac{L}{S}\right) C = \text{const.}$$

on theoretical grounds. Dr. Gladstone considered that the paper contained valuable numerical relationship, and that the second and third formulæ were much more strongly supported by the data given than the first formula. He, the speaker, had previously noticed that the elements of the transition group might be subdivided into sub-groups, and that the elements of each of these sub-groups were particularly closely related. He agreed with the author that gold ought not to be included in the first group. Mr. F. H. Neville said that since the author did not give the source of the data he had employed, most of the results given were rather indefinite. For example, while the author gives 870° as the melting-point of aluminium, Mr. Haycock and himself had found the value 927°. The value of the latent heat of aluminium given was 29·3, while Pionchon, in a recent paper in the *Comptes rendus*, gives the value 80. Theoretical considerations appear to indicate that 80 is the minimum value possible. The author assumes that when you heat a substance from the absolute zero to its melting point, all the energy supplied is expended in the work of expansion. Some of the heat, however, must be employed in changing the kinetic energy of the molecules, even in the case of a solid. Prof. Worthington said that in some cases the amount of work done against cohesive forces between 0° and 100° was much less than one ten-thousandth of the whole amount of energy supplied. Mr. Griffiths said he did not believe in any generalisation which depended on the values of the specific heats determined between 0° and 100°, the rate of change with temperature of specific heat being so great. The author in his reply said he had made every endeavour to obtain the most accurate data for his calculations. The value 29·3 for the latent heat of aluminium was obtained from a paper by J. G. Richards.—A paper on an electromagnetic effect, by Mr. F. W. Bowden, was postponed till the next meeting.

Entomological Society, June 5.—The Right Hon. Lord Walsingham, F.R.S., Vice-President, in the chair.—Dr. Sharp, F.R.S., exhibited, on behalf of Dr. G. D. Haviland, two species of *Calotermes* from Borneo, the individuals being alive and apparently in good health; one of the two small communities (which were contained in glass tubes) consisted of a few individuals of the immature sexual forms and of a neoteinic queen; this latter had increased somewhat in size during the eight months it had been in Dr. Haviland's possession, but no eggs had been deposited, neither had any of the immature individuals developed into winged forms. The second community exhibited consisted entirely of the immature sexual forms, and this community had produced numerous winged adults while it had been in Dr. Haviland's possession. Specimens were also exhibited to illustrate the neoteinic forms that were produced in Borneo after a community had been artificially orphaned. As regards these, Dr. Sharp expressed the hope that Dr. Haviland would shortly publish the very valuable observations he had made. In the case of a species of fungus Termite, Dr. Haviland had found that the community had replaced a king and queen by normal, not by neoteinic forms.—Mr. McLachlan, F.R.S., exhibited examples of the female of *Pyrrhosoma minium*, Harris, having the abdomen incrusted with whitish mud through ovipositing in a ditch in which the water was nearly all dried up. He had noticed the same thing in other species of Agrionidæ.—Mr. Roland Trimen, F.R.S., exhibited some specimens of "Honey" Ants, discovered at Estcourt, in Natal, about a year ago, by Mr. J. M. Hutchinson. The species has not been identified, but is quite different from *Myrmecocystus* and *Camponotus*—the genera which have long been distinguished as containing species, some of whose workers are employed as living honey-pots for the benefit of the community. The specimens exhibited included six "globulars"—to use Mr. McCook's term in regard to the American species, *Myrmecocystus hortus-deorum*—all with the abdomen enormously distended with nectar; but other examples presented to the South African Museum by Mr. Hutchinson comprised various individuals exhibiting different gradations of distension, thus indicating that the condition of absolute repletion is arrived at gradually, and may possibly be reached by some few only of those individuals who feed, or are fed, up for the purpose. Certainly, in the nests examined by Mr. Hutchinson, in Natal, the number of "globulars" was very small in proportion to the population of ordinary workers; and it is somewhat difficult to understand of what particular value as a food reserve so very small a quantity of nectar so exceptionally stored can be. Mr. Trimen added that while the occurrence of "Honey" Ants in Southern, North America, South Australia, and he believed also in India, was well known, the Natal species now exhibited was the first African one that had come under his notice.—Dr. Sharp exhibited a series of Coleoptera, to illustrate variation in size.—Herr Brunner von Wattenwyl made a communication informing the Society that a most unfortunate error had crept into the table of genera in his Monograph of *Pseudophyllides*; on page 9, line 1, and on page 13, line 37, instead of "mesonotum" should be read "mesosternum."

Geological Society, June 5.—W. H. Hudleston, F.R.S., Vice-President, in the chair.—On a well-marked horizon of Radiolarian rocks in the Lower Culm Measures of Devon, Cornwall, and West Somerset, by Dr. G. J. Hinde and Howard Fox. In the Lower Culm Measures the basal *Posidonomya*-beds and the Waddon Barton beds with *Goniatites spiralis* consist of fine shales with thin limestones, and above these are the beds which form the subject of the present paper. The Upper Culm Measures consist of conglomerates, grits, sandstones, and shales, with occasional beds of culm. There is evidence of the partial denudation of the radiolarian rocks during the accumulation of the Upper Culm beds, as indicated by the presence of pebbles of the former in the latter. The radiolarian beds consist of a series of organic siliceous rocks—some of a very hard cherty character, others platy, and yet others of soft incoherent shales. The term "grits," which has been used in connection with these beds, is a misnomer; there are beds which are superficially like fine grits, but they are found to be radiolarian deposits. At present there are not sufficient data for estimating the thickness of the radiolarian deposits; but they are probably some hundreds of feet thick, though the whole does not consist of beds of organic origin. In a quarry in the Launceston district 50 feet of radiolarian cherty rock are seen without admixture of shale. A detailed description of the lithological characters of the rocks of the series was given, and analyses by Mr. J. Hort Player; a marked feature of their composition is the very general absence of carbonate of lime. The microscopic characters of the rocks were also described, and the small amount of detrital matter in the beds of the series was noted. The fossils tend to confirm the view that the Lower Culm Measures are the deep-water equivalents of the carboniferous limestone in other parts of the British Isles, and not shallow-water representatives of deeper beds occurring to the north, as was formerly supposed. In connection with this it was noted that the deep-sea character of the Lower Culm of Germany, which corresponds with our Lower Culm Measures, was maintained by Dr. Holzapfel even before the discovery of radiolaria in the beds of Kieselschiefer furnished such strong evidence in support of this view.—The geology of Mount Ruwenzori and some adjoining regions of Equatorial Africa, by G. F. Scott-Elliot and Dr. J. W. Gregory. Ruwenzori is a mountain between the Albert and Albert Edward Nyanzas. Topographically it is a narrow ridge which extends for about 50 miles in a direction from north-north-east to south-south-west. Its summit attains a height of 16,500 feet. The western slope is at an angle of 22°; the eastern slope at about one of 4°. The authors described sections across the ridge at right angles to its trend. These showed that Ruwenzori is not volcanic, nor is it a simple *massif* of diorite. Epidiorite occurs only as banded sheets in the schists on the flanks of the mountain, and is not the central rock of the ridge. The strike of the flanking schists seems to run concentrically round the ridge as though the central rock were intrusive into them. The highest rock collected, a coarse-grained granite or granitoid gneiss, may be an intrusive igneous rock, but it may be part of the old Archæan series faulted up; there is nothing in its microscopical characters to separate it from the Archæan rocks, and the authors thought it probable that this rock was raised into its present position by faulting. In this case Ruwenzori is simply composed of an orographic block or "scholl," which was at one time probably part of a wide plateau of Archæan rocks. There is abundant evidence of volcanic action around Ruwenzori, for the plains, especially to the east and south-east, are studded with small volcanic cones, arranged on lines which radiate from Ruwenzori. Evidence points to the former occupation of the Nyamwamba, Mubuku, and Batagu valleys by glaciers, *roches moutonnées* of typical character having been noted in the two former valleys. The country round Ruwenzorl consists of rocks which may be conveniently grouped into two series—one composed of gneisses and schists, and the other of non-foliated sediments. The former (the Archæn series) are of the type that has an enormous extension in Equatorial Africa, and forms the main plateau on which all the sediments and volcanic rocks have been deposited. The sedimentary rocks are probably Palæozoic, possibly pre-carboniferous, but in the absence of fossils it would be unsafe to go beyond this statement.—On overthrusts of tertiary date in Dorset, by A. Strahan. The results given in this paper were obtained during a re-survey of South Dorset on the 6-inch scale. The disturbances can be divided into two groups—the one being mainly of Miocene date, and the other of intra-cretaceous (between Wealden and Gault) age. The former includes the Isle of Purbeck fold (which is the continuation of the Isle of Wight disturbance), the Ringstead fold, the Chaldon and Ridgeway disturbances, and the Litton Cheney fault. In the latter are placed the anticline of Osmington Mill, the syncline of Upton, and a part of the anticline of Chaldon; farther west the Broadway anticline and Upway syncline, a fault at Abbotsbury, and many other folds come into the same group. These earlier movements led to the well-known unconformity at the base of the Upper Cretaceous rocks.

Linnean Society, June 6.—Mr. W. Percy Sladen, Vice-President, in the chair.—The minutes of the last meeting having been read and confirmed, the Chairman, on behalf of the President, declared the following to be Vice-Presidents:—Messrs. J. G. Baker, F. Crisp, A. Lister, and W. P. Sladen. Mr. B. B. Woodward was elected a Fellow.—Mr. M. Buysman, who has laboured for many years to establish a garden at Middleburg for economic plants, exhibited specimens to show the excellence and completeness of his preparations.—On behalf of Mr. T. J. Mann, who had lately returned from Ceylon, Mr. Harting exhibited specimens of a butterfly, *Catophaga galena*, Felder, which had been observed migrating in thousands across the northern part of the island during March and April last, in a direction from north-east to south-west. The movement commenced about 7 a.m. and lasted until noon, when it decreased, and was resumed in the afternoon for another two hours. Mr. Harting referred to the remarks on this subject made by Sir J. Emerson Tennent ("Nat. Hist.," Ceylon, 1861, p. 404, note) to the observations of Darwin on the countless myriads of butterflies met with at sea some miles off the mouth of the Plata, and to a paper by Mr. R. McLachlan on the migratory habits of *Vanessa cardui* (*Entom. Mo. Mag.*, xvi. p. 49). He did not think that the movement was analogous to the migration of birds which migrated in opposite directions in spring and autumn, for the insects moved only in one direction, and did not return, vast numbers perishing *en route*. The phenomenon rather resembled what had been observed in the case of lemmings, locusts, and dragon-flies (Weissenborn, *Mag. Nat. Hist.*, n.s., vol. iii. p. 516), and might be explained as a sudden exodus from the birthplace, leading to a compensating reduction of the species after a season exceptionally favourable to its increase. His remarks were criticised by Colonel Swinhoe, who was inclined to confirm this view, and by Mr. Kirby, who referred to the particular species which were found to take part in these so-called "migrations."—A new *Distomum* was described by Mr. G. West, whose observations were favourably criticised by Mr. W. P. Sladen and Prof. Howes.—On behalf of Mme. van der Bosse, Mr. George Murray communicated a description of a new genus of Algæ (*Pseudocodium*), the characters of which were minutely pointed out by means of specially-prepared lantern slides.—A paper was then read by Mr. A. Vaughan Jennings on the nature of *Mobiusispongia parasitica*, on which critical remarks were made by Prof. Rupert Jones and Mr. F. Chapman.—A second paper by Mr. Vaughan Jennings contained a description of a new genus of Foraminifera of the family *Astrorhizidæ*.

PARIS.

Academy of Sciences, June 10.—M. Lœwy in the chair.—On the Meudon Physico-Astronomical Observatory, by M. J. Janssen. An account of the present state of the Observatory and of the difficulties through which it has passed on account of the reductions made in the State grants and appropriations, together with some details of the work done since 1876.—On the necessarily harmonic form of expression, for the displacements of each particle in an ocean roller, as a function of the time, by M. J. Boussinesq.—Note on the photographic surveys executed in 1894 by the Canadian engineers and the United States Coast and Geodetic Survey Service for the delimitation of Alaska and British Columbia, by M. A. Laussedat. This is an account of the spread of the Canadian method into the United States Service, and a review of the general adoption of similar processes in other countries.—Solar observations made at Lyons Observatory during the first quarter of 1895, by M. J. Guillaume.—On algebraical curves of constant twist and on algebraical minima surfaces inscribed in a sphere, by M. E. Cosserat.—New theorems in arithmetic, by P. Pepin.—On an explosive system capable of demonstrating the rotation of the terrestrial globe, by M. Jules Andrade.—Spectroscopic study of carbons from the electric furnace, by M. H. Deslandres. Two carbon poles used in M. Moissan's electric furnace were examined. Those parts of the carbon removed from the arc showed the usual spectra of impurities, whereas the parts in the arc were free from all impurities except calcium. The growths on the negative pole were of greatest purity, and furnished carbon spectra showing wave-lengths (cited) much fewer than those recorded for carbon by Hartley and others. The purification of the carbons by the passage of the current in the arc is due to the volatilisation of the more volatile constituents at the high temperature obtained.—On sensitive flames, by M. E. Bouty.—Physical properties of acetylene; acetylene hydrate, by M. P. Villard. A table of pressures corresponding to certain temperatures is given for acetylene, together with a table of dissociation pressures for the hydrate of acetylene. This hydrate resembles the hydrates of nitrous oxide and carbon dioxide, and is represented as $C_2H_2.6H_2O$. Its heat of combination is 15·4 Cal. per molecule, very near to the value, 15·0 Cal., obtained for carbon dioxide and nitrous oxide.—Synthetical production of nitro-alcohols, by M. Louis Henry.—Condensation of aldehydes and saturated ketones, by MM. Ph. Barbier and L. Bouveault. The researches detailed appear to establish the fact that only ordinary acetone can condense easily with aldehydes; on the other hand, as the molecular weight of the aldehydes increases, the aptitude for condensation with acetone diminishes, and the principal reaction becomes the condensation of the aldehyde itself.—On the causes of the colouration and the coagulation of milk by heat, by MM. P. Cazeneuve and Haddon. The conclusions are drawn: (1) That the yellowing of milk by heat is due to oxidation of lactose in the presence of the alkaline salts of the milk; (2) the oxidation of lactose produces acids, formic among others, and hence causes coagulation of the milk; (3) the coagulated casein is not itself altered, but is merely tinted by the decomposition products of lactose.—Esters of the active α-oxybutyric acids, by MM. Ph. A. Guye and Ch. Jordan.—On the history of the alkaloids of the Fumariaceæ and Papaveraceæ, by M. Battandier.—Contribution to the study of germination, by M. Th. Schlœsing. The germination of lupin seeds or wheat does not entail an appreciable loss of nitrogen in the gaseous state.—On amylase, by M. Effront.—The Cecidomyia of oats (*Cecidomyia avenæ*, nov. sp.), by M. Paul Marchal.—The epidermal cell of insects: its paraplasm and its nucleus, by M. Joannes Chatin.—On the relation of the thermal springs of Néris and Évaux with ancient faults of the Central Plateau, by M. L. de Launay.—On the succession of fauna of the Upper Lias and Lower Bajocien in Poitou, by M. Jules Welsch.—Researches on the modifications of nutrition in persons suffering from cancer, by MM. Simon Duplay and Savoire. The differences observed in amounts of urea and phosphoric acid excreted by cancerous patients, as compared with the normal healthy excretion, are due entirely to defective nutrition, and disappear when a suitable régime is used. These differences cannot be used for purposes of diagnosis.—On the use of chloride of lime and its mode of action against the bite of venomous serpents, by MM. C. Phisalix and G. Bertrand.—Storms of five days from May 20 to May 25, 1895, in Bohemia, by M. Ch. V. Zenger.

BOOKS, PAMPHLETS, SERIALS, &c., RECEIVED.

BOOKS.—A Chapter on Birds. Rare British Visitors: Dr. R. B. Sharpe (S.P.C.K.).—The Metallurgy of Iron and Steel. Vol 1. The Metallurgy of Iron: T. Turner (Griffin).—The Story of the Plants: Grant Allen (Newnes).—England's Treasure by Foreign Trade: T. Mun, 1664 (Macmillan).—Natural History of Aquatic Insects: Prof. L. C. Miall (Macmillan).—Chemistry, Inorganic and Organic: C. L. Bloxam, 8th edition, rewritten and revised by Thomson and Bloxam (Churchill).—The Great Frozen Land: F. G. Jackson (Macmillan).

PAMPHLETS.—Report of the Director of the Observatory to the Marine Committee, Liverpool Observatory, Bidston, 1894 (Liverpool).—Les Variations Périodiques des Glaciers des Alpes, Report, 1894: Prof. Forel (Berne).—White Servitude in the Colony of Virginia: J. C. Ballagh (Baltimore).—Protection from Lightning: A. McAdie (Washington).

SERIALS.—American Naturalist, June (Philadelphia).—Journal of the Franklin Institute, June (Philadelphia).—Abstract of Proceedings of the South London Entomological and Natural History Society, 1894 (London).—Seismological Journal of Japan, Vol. 4 (Yokohama).—Mathematical Gazette, May (Macmillan).—Mémoires de la Société de Physique et D'Histoire Naturelle de Genève, tome xxxii. Première Partie (Genève).—Kew Observatory, Richmond, Report for the Year 1894 (Harrison).—Bulletin of the Geological Institution of the University of Upsala, Vol. 2, Part 1, No. 3 (Upsala).—Massachusetts Institute of Technology, Boston, Annual Catalogue, 1894-95 (Cambridge, Mass.).

Betts's Chromoscope (Philip).

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